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ARTICLES

INTIMIDATION AND THE SYMBOLIC USES OF TERROR IN THE USSR

DONNA BAHRY
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BRIAN D. SILVER
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Theories of regime-society relations in Communist states stress the central role of coercion in maintaining political control. Based on a survey of Soviet emigrants, we examine whether Soviet citizens are deterred from nonconformity by the punitive actions of the KGB (individual deterrence), a perception of the KGB's coercive potential (general deterrence), or mistrust of other people. We find that few respondents were directly coerced by the KGB (and those who were had engaged in the most serious kinds of nonconformity); that those who had punitive contacts with the KGB in the past were not deterred from subsequent nonconformity; that the KGB's competent image was a general deterrent; and that trust in other people facilitated both nonconformist and *compliant* political activism. *Those who came of political age under Khrushchev and Brezhnev were more likely to be involved in both kinds of activism than those who came of age under Stalin.*

The feature of the Soviet political system that is most often said to distinguish it from industrial democracies is the use of state terror—physical coercion or the threat of it—to assure compliance with the regime and to remake the nature of man. In his characterization of the Soviet system under Stalin, Merle Fainsod wrote, "Terror is the linchpin of modern totalitarianism" (1961, 384). In their classic book, *Totalitarian Dictatorship and Autocracy,* Carl Friedrich and Zbigniew Brzezinski (1965) described a terrorist police as one of six key features of the "totalitarian syndrome." Totalitarian states are said to atomize society so that people become isolated and mistrustful of one another and hence unable to concert their efforts in organized political activity (cf. Kornhauser 1959). They fear the *stukach* (stool pigeon) and the possibility that even an unguarded or innocent remark could be used against them.

The actual use of state-sponsored terror as an instrument of control has unquestionably decreased, however, since Stalin's time. The reduction is reflected not only in the decline in the use of physical force for political ends but also in reduced autonomy and authority of the secret police agency itself. Even though in the late 1970s the status of the KGB (Komitet gosudarstvennoi bezopasnosti, "Committee on State Security") was upgraded in visible ways and the number of dissidents arrested increased (Knight 1980; Reddaway 1983), overt mass terror is much less common now than in Stalin's time.

Whether a Soviet system with limited overt use of terror should be classified as something other than totalitarian has exercised the minds of many scholars.

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Although few question that the Soviet state has reduced its overt use of force for political ends since Stalin's day, most recognize that the main instruments of political control remain in place. For example, Bialer (1986) has argued that there are three main reasons why the Soviet system survived the abolition of terror: the inhibiting effect of the memory of the Great Terror among the generations brought up under Stalin; the maintenance of a political police that uses more measured and predictable, but nonetheless effective, measures of control; and the use of additional means of nurturing mass support, such as material incentives.

This suggests that it is the style of coercion that has changed in the Soviet Union, not the essential commitment to use it to enforce political orthodoxy. The argument is not simply that the Communist party has cultivated other means of generating mass compliance, such as material incentives, but that a mature coercive regime does not need to engage in constant harassment of every person who strays from the orthodox line. It can tolerate unorthodox thoughts and a certain amount of unorthodox behavior, while resorting to coercion only when an unorthodox activity intensifies or challenges the authority of the regime.

More important, an efficient system of controls can rely on a perception of its coercive potential to inhibit deviation from prescribed political norms. As Jeremy Azrael has observed, in the post-Stalin era more of the activities of the secret police are "designed to have a 'prophylactic' instead of a 'punitive' effect" (1962, 10). From this perspective, it is important that the state be perceived as having the capability and will to uncover and to act against dissident or unorthodox activity, whether or not it actually does so in every instance.[1]

An equally important instrument of collective coercion against the individual according to the totalitarian model is the individual's lack of trust in others, a sense that risks lurk not only in the overt activities of the agencies of coercion but also in one's most ordinary contacts with coworkers, bosses, friends, and even relatives. It is this set of perceptions—of the coercive potential of the political police and of other people—that we call the *intimidation factor.*

We use information provided by former Soviet citizens to study both perceptions of the KGB and interpersonal trust and to test propositions about the relation between these perceptions and political behavior. Did the experience of direct intimidation by the KGB deter the individual from engaging in subsequent unorthodox behavior? What is the KGB's image? Is the KGB held in higher regard than other Soviet institutions? How prevalent is social atomization? Were those who were less trusting of others likely to take fewer chances and to engage less in behavior that the regime considers unorthodox?

We focus on the KGB as the primary agency of coercion in the USSR. Other institutions, of course, also play a role in intimidation. We study the image of the KGB because it represents the most severe coercive agency. Those who are not intimidated by the secret police are unlikely to be intimidated by other, less coercive institutions.

The Soviet Interview Project General Survey

The data for this study come from the Soviet Interview Project (SIP) general survey of 2,793 former Soviet citizens. Most of the questions on the survey centered on the respondents' "last period of normal life" (LNP) in the USSR—the five years prior to the major disruption associated with their decision to emigrate. For most survey respondents, this was when Leonid Brezhnev was general secretary of the Communist party and Yuri Andropov was the head of the KGB.[2] The respondents arrived in the United States between 1 January 1979 and 30 April 1982 and were between the ages of 21 and 70 on the date of their arrival. They were interviewed between March 1983 and January 1984.[3]

Can the results of the SIP general survey yield valid insights about mass politics in the USSR? We would not suggest that either the overall distributions or summary statistics from a survey of Soviet emigrants be generalized directly to the Soviet population, especially because the respondents come overwhelmingly from large cities and are (by definition) emigrants and because about 85% of them are Jewish by some definition.

Our concern, however, is with the pattern of relations among variables, such as the relation between people's image of the KGB and their compliance with the Soviet model of a politically mobilized citizen (cf. Zimmerman 1987). And our ability to generalize is limited to the Soviet adult population from large Soviet cities, the "referent Soviet population" (Anderson and Silver 1987a). Previous research using SIP data confirms that such findings do reflect Soviet reality (Bahry 1987). For instance, the connections between socioeconomic characteristics and political activity essentially match those reported in Soviet surveys. So, too, does the pattern of early recruitment into the Komsomol for different generations and social groups. Friedgut's (1979) study based on Soviet emigrants supports a similar conclusion.

In addition, previous research shows that most of the responses to questions on political attitudes and behavior are not sensitive to whether the respondents were Jews or to whether they had especially traumatic experiences associated with emigration. Thus, although one can never prove that all the patterns in the responses reflect the attitudes and experiences of the referent Soviet population, the results are not sensitive to some of the obvious candidate sources of bias.[4]

Theoretical Background

Conceptual Insights from the Theory of Deterrence

The literature on deterrence, particularly as it has been applied to understanding law enforcement, provides a useful conceptual framework for understanding the effects one should expect from government coercion, though we do not wish to press the analogy between ordinary crime control and the activities of political police.[5]

When agencies of the state resort to force against people, their usual immediate objective is to suppress the activities themselves. By arresting or punishing people, they try also to affect their future behavior or that of others. First, they can *incapacitate* people—remove them from society—by detaining, imprisoning, exiling, or killing them. Second, they can seek *individual deterrence,* by retaliating against nonconformists and making them hesitant in the future to repeat the same or other undesirable actions or by inducing them to behave in more appropriate ways in future. Third, they can try to foster *general deterrence,* by making other people hesitant to become involved in undesirable activities. General deterrence depends on shaping peoples' perceptions of what could happen to them if they were to overstep the boundaries of acceptable behavior.

Individual Deterrence. The shift in the KGB's role in Soviet society from the 1930s to the present involves a shift from relying heavily on incapacitation to relying more on individual and general deterrence. The literature on law enforcement suggests that the individual is deterred from straying because of both the certainty and the severity of punishment. In general, however, high certainty of punishment is a more effective deterrent than high severity of punishment. The logic, supported by some empirical evidence, is that increasing the penalty is not likely to deter deviant behavior unless people think they are likely to be caught (see Piliavin, et al. 1986).

By this same logic, the KGB does not need special tribunals and a vast archipelago of labor camps to be an effective political police. But it does need to make people believe that if they do step out of line, retribution will be nearly certain. This logic applies to both individual and general deterrence.

General Deterrence. For general deterrence to work, the agencies of coercion must appear to be effective and the punishment meted out must be visible, publicized. Or the memory of past activities of the agencies must be strong. A perception that some activity might be risky is only a deterrent if reprisal is very likely.

Evidence among SIP general-survey respondents supports this distinction. Often, people who were aware of the risks of engaging in unconventional behavior nonetheless did it. For example, 30% of the respondents reported that they read *samizdat* or *tamizdat* (unauthorized materials in the USSR);[6] of those who did not read it, less than 1% refrained from doing so explicitly because it was too risky. On the other hand, of the 30% who did read *samizdat,* 84% thought it was risky to do so, and 40% of the latter classified it as very risky.

In fact, however, only 1 of the 280 respondents who claimed to have read *samizdat* during their LNP reported being punished for doing so. Thus, this form of unconventional behavior was not deterred by the perception of risk alone. Those who did not engage in it seldom volunteered its riskiness as a reason; they were much more likely to mention that *samizdat* was not available or that they were not interested in it. At the same time, those who did engage in the behavior did so despite perceiving the potential hazards.

The relation between the perception of risk and type of *samizdat* that people read is instructive. Those who read literature about human rights and the regime's political abuses regarded the reading of *samizdat-tamizdat* as much riskier than those who read nonpolitical materials, even though punishment was rare in both cases. Ironically, politically oriented materials may send an unintended message that reinforces the image of the potency and ruthlessness of coercive agencies. Materials that chronicle state abuses remind readers of the state's power and the heavy costs to be paid by those who fail to comply. And articles and books critical of the vast majority of compliant citizens tacitly underscore the social isolation of political dissidents (Jahoda and Cook 1954).

Random Terror. Scholars writing on terror in the USSR have emphasized the importance of random terror as a method of totalitarian control. According to this idea, it is not the predictability and certainty of punishment that deters people from engaging in unconventional behavior, but instead its unpredictability and uncertainty. Brzezinski puts it thus: "It is this selective, unfathomable character of the purge which gives it its effectiveness" (1956, 28). Alexander Dallin and George Breslauer note that "it is this element of arbitrariness—or, from the vantage point of the citizen, the unpredictability—in the use of terror that is [terror's] distinguishing mark" (1970, 4).

We think the appearance of randomness or arbitrariness with respect to the *targets* of terror is most effective at given stages in the development of a regime, in what Dallin and Breslauer (1970) refer to as the *takeover* and *mobilization* stages. Random terror is especially relevant to a regime that is committed to undermining the established social order. As Barrington Moore has observed, "Capricious or arbitrary use of terror undermines the essential basis of social organization" (1954, 170).

At a later stage of development of the regime, however, not only are other mechanisms of achieving compliance available, but utter arbitrariness in the application of coercion punishes people who are supportive of the system. Furthermore, it is not efficient for the police either to try to achieve 100% compliance or to punish every transgressor of official norms.

An analogous argument has been presented in the context of the strategy of two-person games. Assuming that agencies of coercion must be selective in the application of coercion, it is better to be unpredictable than not.[7] In this way, it is more difficult for those who wish to violate the established norms to work the system to their own advantage, like knowing when the cop on the beat makes his rounds. Under uncertainty, the prospective wrongdoer will be deterred from acting because the risk of punishment becomes incalculable.

The usual objective of the person who threatens coercion is to modify the behavior of the other person in a specific way, not to terrorize him into total inaction. For this reason, enforcement should not be random or unpredictable with respect to the types of behavior that are sanctioned. That is, which individuals will be caught and punished may not be highly predictable; but what kinds of activities will be punished should be more certain.

Following this line of argument, one would not expect to find certain and severe punishment of all transgressors of official Soviet political norms. Nor would the lack of universal and harsh enforcement of official norms of behavior be a sign of a lack of capability or will of the political elite. But one would expect actions that are more threatening to the regime to be more consistently punished than those that are not. And leaders of unsanctioned activities should be more likely to be punished than those who are only participants.

Social Atomization. A related component of deterrence that has been especially emphasized in theories of totalitarian control is the deliberate undermining of social bonds among individuals. Society itself thereby becomes an instrument of coercion: the memory of mass terror, the elimination of autonomous intermediary groups between state and individual, and the continued reliance on informers breed an atmosphere of social intimidation that undermines any collective activity not officially sanctioned by the state. Moore outlined the process well:

> The regime deliberately seeks to sow suspicion among the population, which to a marked extent results in the breakup of friendship groupings, in the work situation and elsewhere, and the isolation of the individual. . . . Terror ultimately destroys the network of stable expectations concerning what other people will do that lie at the core of any set of organized human relationships. Dissent may thus be deterred not so much by the direct threat of punishment by the KGB (although it looms in the background) but by a generalized fear or mistrust of others, a lack of faith in people (1954, 158, 164).

Thus, based on the scholarly literature on deterrence and on terror in totalitarian systems, there is reason to ask both when and how coercion comes into play, and whether the actual or expected application of negative sanctions works as either an individual or a general deterrent. How likely is it that people will in fact be punished? And does punishment or the anticipation of punishment actually deter people from political nonconformity?

Results

Is Coercion Random?

Previous research based on SIP general-survey data has shown that only a small proportion of the respondents engaged in overt political nonconformity.[8] By and large, they were quite conventional, judging by both the standards established by the Soviet government and the patterns of political participation found in other developed countries.

Because of the small number of respondents to the survey who engaged in specific unconventional political activities, we must be cautious about interpreting the patterns of regime response to each kind of noncompliance. In addition, we do not have information about the frequency with which individuals engaged in each of the behaviors. Usually, we know *whether* they engaged in different types of behavior in their LNP, but not how often.

We have better evidence about the level of involvement of the individual in various kinds of unconventional activity, that is, whether the respondents were leaders in the activity or were only participants in it. And for those activities, we can assess whether involvement led to punishment by the authorities.

As shown in the top panel of Table 1, those who were leaders of unconventional activities were consistently more likely to be punished for their activity than those who were merely participants. Table 2 shows that, of all nonconformist leaders, 50% were punished.[9] The probability that they were punished increased with the variety of activities that they led. In contrast, of all those who were participants (but not leaders) in the same kinds of activities, only 7% were punished, though the proportion penalized also increased with the number of different types of activity in which people participated. Reprisals were especially likely for those who engaged in more serious, public threats to the regime, for example, strikes and overt protests.

From this evidence, one would conclude that punitive action by Soviet authorities is very predictable.[10] The question remains, however, whether those who were *not* involved in overt unconventional behavior during the LNP were also targets of the KGB. If the use of coercion were random with respect to the kinds of activities in which people engaged, the secret police would have intervened directly in the lives of many people who did not knowingly engage in any overt unconventional behavior.

Only 2% ($N = 58$) of the SIP general-survey respondents reported such contact with the KGB during their LNP.[11] One-fourth of this small group were asked by the KGB to help it conduct *its own* business: the KGB solicited the respondents as witnesses in an investigation or asked them to provide information about other people. Other encounters with the KGB related to the *respondent*'s activities: 24% concerned contact with foreigners; 17%, illegal economic activities; 12%, other restrictions on the respondents' freedom of speech or movement (e.g., the respondents' reading of *samizdat*, complaining to the government, "saying the wrong thing," visiting a place where they were forbidden to go); 5%, ethnic reasons or engaging in religious observances; and 17%, unidentified reasons.

Direct contact with the KGB was therefore minimal for those who refrained from unconventional activities in the LNP.[12] Not surprisingly, the KGB was far more likely to intervene in the lives of those who publicly threatened the established political order. For example, out of approximately 20% of all respondents who reported not voting during their LNP, the only ones punished for it were individuals who also took part in overt unconventional activity.

A similar pattern holds with respect to

regime responses to individual demands and complaints (see Table 1). The vast majority of citizen-initiated contacts with Party and state agencies, mostly concerned with pocketbook issues and public services, evoked either a positive response or no response from the agency contacted. Only a handful led to reprisals, and these were for contacts that were more overtly political, such as complaints about the state itself or its handling of citizen rights.

Table 1. Punishment by Authorities for Political Activities in the LNP, by Type of Activity

Activities	Percentage Punished[a]	Base Number of Cases
Unconventional political activities		
Unofficial study group		
Participant	4.2	166
Leader	18.8	16
Distribution of *samizdat*		
Participant	8.8	102
Leader	23.1	13
Overt protest		
Participant	23.1	26
Leader	100.0	5
Strike		
Participant	13.0	23
Leader	55.6	18
Other self-defined activity[b]		
Participant	16.7	6
Leader	64.3	14
Other unconventional activities		
Nonvoting		
Sometimes	1.6	506
Always	1.7	292
Attending religious services	1.1	888
Read *samizdat–tamizdat*[c]	.4	262
Citizen-initiated contact with authorities[d]		
Contacted government or party official		
Material reason	2.4	405
Administrative, civil matter	2.8	69
Protest	28.0	25
Contacted media		
Material reason	5.1	59
Administrative, civil matter	.0	8
Protest	27.8	18

[a] *Punishment* is any official action taken against the respondent for the particular activity the respondent reports.

[b] "Other" unconventional activities are defined by respondent. The most frequently mentioned were teaching/study of Hebrew/Yiddish; involvement with dissident movement; and illegal economic activity.

[c] The question about reading of *samizdat* or *tamizdat* was asked only of a random one-third of the respondents.

[d] These items included citizen-initiated contacts of all types, and are recoded by the authors into the categories listed. Many such contacts evoked positive responses, and many evoked no response.

Table 2. Punishment by Authorities, by Intensity of Involvement in Politically Nonconformist Activities

Number of Activities	Participant		Leader	
	Percentage Punished[a]	Base Number of Cases	Percentage Punished[a]	Base Number of Cases
1	5.7	176	44.7	38
2	16.4	55	50.0	8
3	38.9	18	100.0	3
4	20.0	5	100.0	1
5	50.0	2	—	0
1 or more	7.2	256	50.0	50

Note: *Political nonconformity* includes participation during the last normal period in an unofficial study group, distribution of *samizdat,* overt protest, strike at work, or any other unsanctioned political activity as defined by the respondent.

[a] In order for a person to be *punished* for any of the activities defined here, he or she must have been either a participant or a leader. Hence, no individual in this tabulation can have engaged in *0* unsanctioned activities.

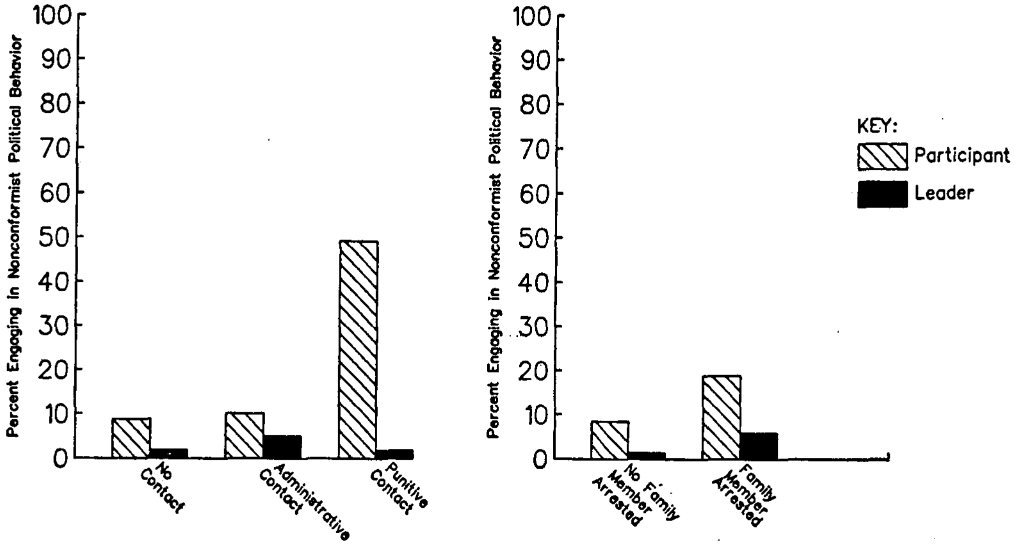
Individual Deterrence

Since most questions in the SIP general survey focused on events and attitudes during the last five years of a respondent's life in the USSR before it was disrupted by plans to emigrate, it is difficult to determine whether people who were punished for unconventional behavior were deterred from doing it again. Fortunately, we do have some critical additional information: whether respondents had been contacted by the KGB earlier in their lives (before the beginning of the LNP), and whether they had a prior history of family arrest (whether someone in their family was arrested before the respondent ever got into trouble for some activity during the LNP). We would expect that if individual deterrence comes into play, those with early contact or family arrest should be less involved in nonconformity.

The data in Figure 1 reveal that such earlier experiences did not necessarily deter further involvement. Individuals penalized earlier by the KGB were almost four times as likely as other respondents to engage in unorthodox political activity during the LNP, although they were not more likely to be leaders.[13] And those who came from families that experienced a political arrest turned out to be twice as likely as others to engage in unconventional activity during the LNP. Evidence on the number of family members arrested provides additional support for this conclusion: the higher the number, the more likely the respondents themselves were to engage in unorthodox political activity (the data are not shown here).

Two explanations for this pattern seem plausible. First, whatever it is that impels people to engage in unorthodox political activity does not necessarily disappear once the person has been punished by the regime. Calling unorthodox political actors to account may be punitive but not rehabilitative or corrective.[14] Second, once individuals are branded as troublemakers, their opportunities for political redemption may be limited. They face continuing extra scrutiny by persons in authority at work, by neighbors, and by social organizations. They may be isolated from and ostracized by others who do not want to be judged guilty by association. At the same time, they may receive social support from others who are similarly stigmatized by the authorities as political deviants, and perhaps

Figure 1. Relation between Early KGB Contact and Political Nonconformity



Respondent's Contact with KGB before LNP

Prior Political Arrest of a Family Member

from Western news media, intellectuals, or politicians.

This does not mean that being punished makes someone more likely than before to engage in unconventional political activity. But the balance of incentives and disincentives to be compliant or nonconformist shifts once the person is identified by the authorities as a nonconformist.

Perceptions of the KGB: Competence, Honesty, and Other Dimensions

The analysis to this point shows that individual deterrence does not necessarily work. For the majority of SIP respondents, however, who did not have direct contact with the KGB, the effect of the KGB on political behavior may depend more on the KGB's image of competence. As Moore has written, knowledge of how many people the regime has arrested is not the only factor shaping compliance and nonconformity: "Particularly in the case of such a phenomenon as terror, popular beliefs about its impact are likely to be more influential in determining actual conduct than the objective facts themselves" (1954, 156).

Accordingly, we now examine two dimensions of the intimidation factor that may serve as a general deterrent on unorthodox behavior: perceptions of the competence of the KGB and of the trustworthiness of other people. A KGB that is perceived as competent is likely to be more of a general deterrent than one perceived as incompetent. People who do not trust one another are also more likely to be deterred from unorthodox behavior than people who have greater faith in other people. In the final section of the analysis, we will test for the effects of these perceptions on the incidence of unorthodox behavior.

Competence and Honesty. Judging from

answers to questions about the competence of leaders of eight key Soviet institutions, the KGB is viewed as highly capable: 57% of all respondents said that almost all or most of the KGB's leaders were competent (*kompetentnyi*) or did their jobs well (see Figure 2).[15] The KGB ranked third in competence among the eight institutions. The highest ratings were given to the leaders of the Academy of Sciences and of the military; 66% of all respondents stated that almost all or most of the leaders of these institutions were competent.

Industrial-enterprise managers were rated fourth, the Politburo fifth. The leaders of local political institutions—the *militsiia* (local police), the local party, and the local government council—were rated much lower. Thus, the responses to

Figure 2. Perceptions of Competency of Leaders in Major Soviet Institutions

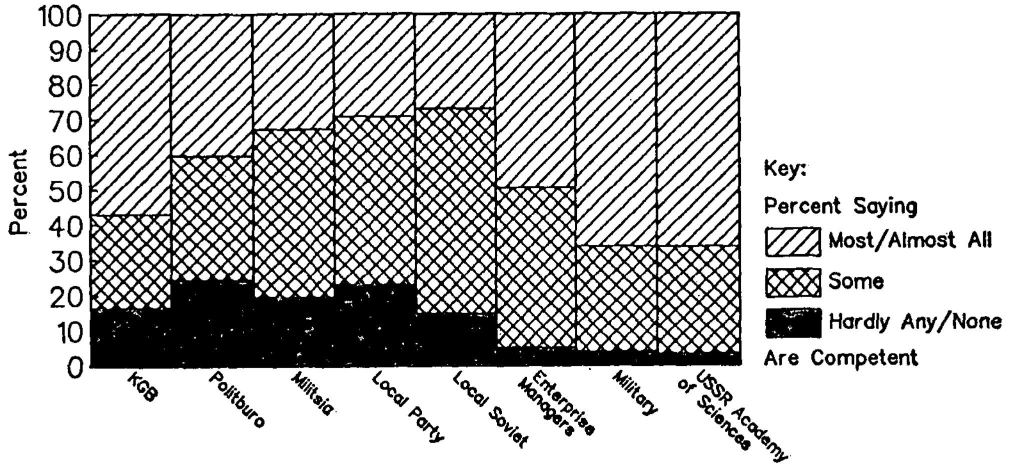
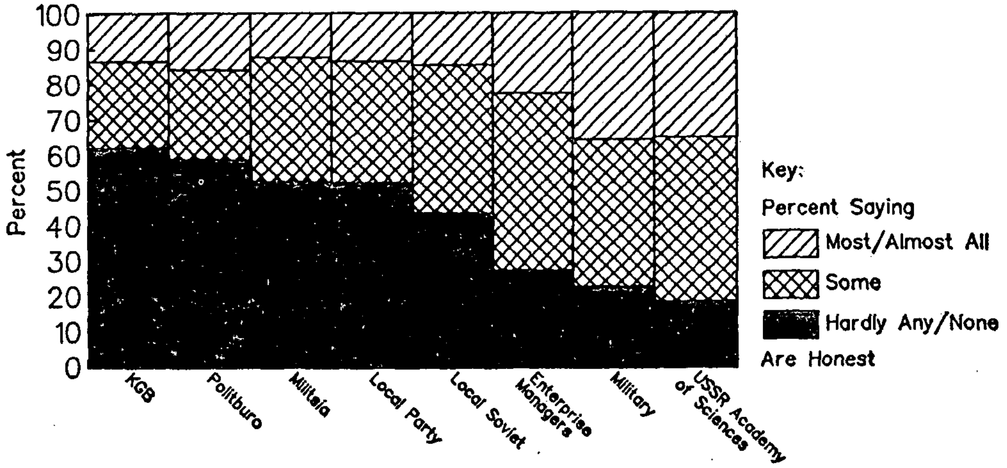


Figure 3. Perceptions of Honesty of Leaders in Major Soviet Institutions

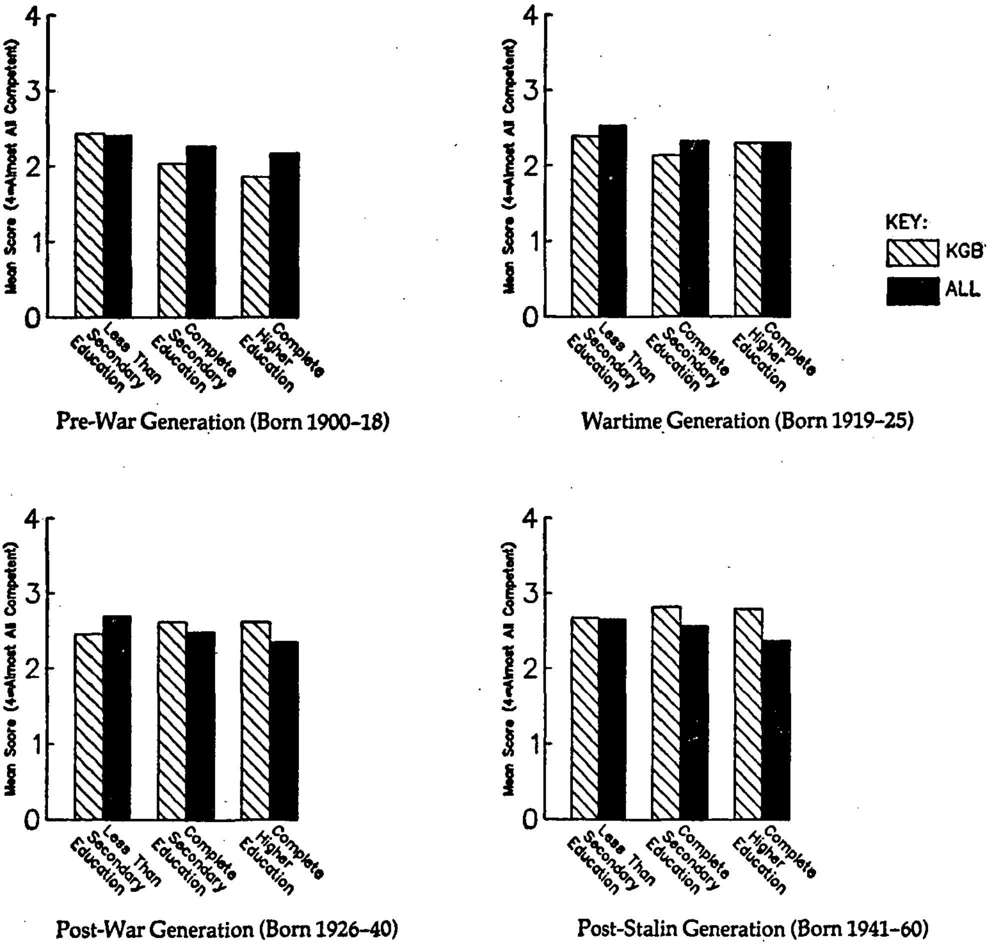


the questions on competence vary considerably, and within the range of variation, the KGB was rated quite highly.

It is possible that the answers to the questions about the competence of Soviet officials reflect a more general evaluation rather than an evaluation of competence per se. To determine whether this was so, we examined responses to questions about how honest or honorable (*chestnyi*) the leaders of the eight institutions were. If the responses to the questions about competence and honesty were substantially different, then we could be more confident that the responses to the questions about competence are judgments of competence rather than of more general qualities of the institutional leaders.

When the questions related to honesty, the leaders of major Soviet institutions were judged much more negatively (compare Figures 2 and 3). The gap between the average competence and honesty ratings for each institution implies that the responses to the two sets of items do not derive from a single underlying

Figure 4. Perceived Competence of Leaders of the KGB and All Major Institutions, by Level of Education within Generations



Pre-War Generation (Born 1900–18)

Wartime Generation (Born 1919–25)

Post-War Generation (Born 1926–40)

Post-Stalin Generation (Born 1941–60)

dimension of evaluation.[16] The leaders of the Soviet military and the Academy of Sciences were again the two most highly rated. Enterprise managers were third. Depending on what part of the distribution one focuses on, the leaders of the KGB rank either sixth or eighth—last—on the *honesty* dimension.

Thus, leaders of major Soviet institutions have a split image. They are viewed as generally capable, but not very honest. Even for the two agencies for which a large majority of the officials were judged to be competent, barely a third of the respondents judged them to be honest. The largest relative gaps between their image of competence and their image of honesty appeared for the Politburo, the KGB, and the *militsiia*. The smallest relative gaps appeared for leaders of the Academy of Sciences, the military, and industrial enterprises.

It is important to emphasize that neither dimension of evaluation should be interpreted as a measure of affect for the leaders of the institutions, particularly of the KGB. One does not admit that one actually likes a hangman when one acknowledges the efficiency with which he manipulates the rope.

In the remainder of the analysis, we examine factors related to the image of the competence of institutional leaders. Our main concern is to study perceptions of the capability or competence of the KGB, and to determine whether these perceptions are related to political nonconformity.

Determinants of Perceptions of Competence. To understand the factors that affect the evaluation of the secret police in particular, it is important to understand first what affects evaluations of authorities more generally. The basic reason for this is that respondents who evaluated the leaders of one institution as competent were also likely to evaluate leaders of the other seven institutions as competent. Table A-3 (see Appendix) shows the correlations between the evaluations of the eight institutions, with the responses to the questions on competence transformed into numeric scores by assigning values to the response categories, ranging from zero (lowest evaluation) to four (highest evaluation).[17] The correlations range from moderately to highly positive.

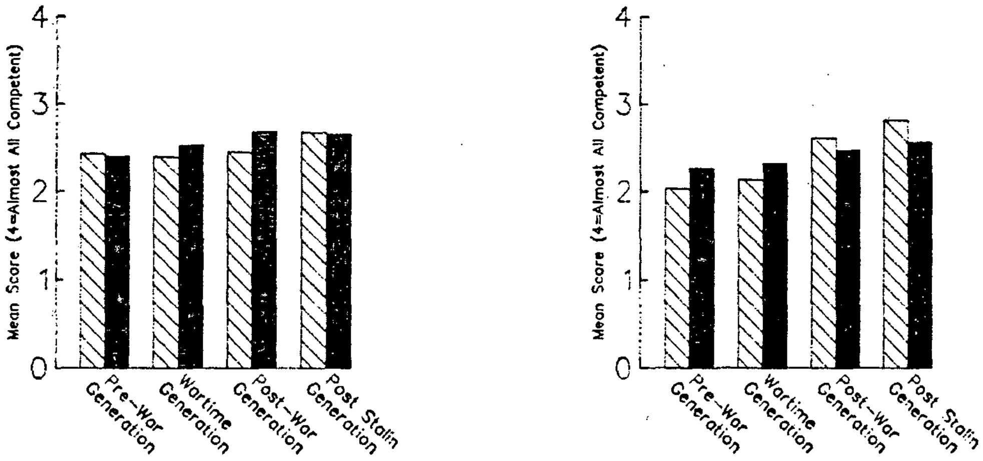
Initially, one might hypothesize that both the level of education and political generation will affect individual perceptions of leaders of major Soviet institutions. Previous research (Silver 1987) has shown that increases in education have tended to undermine support for two Soviet "regime norms," state control of the economy and collective or social control at the expense of individual rights.[18] Also, younger respondents were less likely to support the norms than older ones.

The responses to the questions on competence of the authorities bear out only part of the pattern. As shown in Figure 4, the perceived competence of institutional leaders declines as the respondent's education increases. This is true for all eight institutions (see Table A-1), and within every generation. But generational effects move in the opposite direction. Younger cohorts rate the competence of Soviet leaders more highly than do older ones, even after adjusting for educational differences (see Figure 5 and Appendix Table A-2).

In light of these findings, it is important to examine whether other factors known to be related to support for regime norms are also related to the evaluation of the competence of Soviet institutional leaders. Based on earlier research, one might expect people who were more satisfied with their material status (housing, job, standard of living, provision of medical care) to evaluate the abilities of Soviet officials more highly (Silver 1987). Those more supportive of regime norms (established institutional practices) should also be more likely to evaluate the leaders of the institutions positively. On the other hand, one might expect persons who were more interested in political affairs to have a more negative evaluation of Soviet leaders. Finally, people who have experienced direct punitive contact with the KGB prior to their LNP ought to hold a more negative view of the authorities.

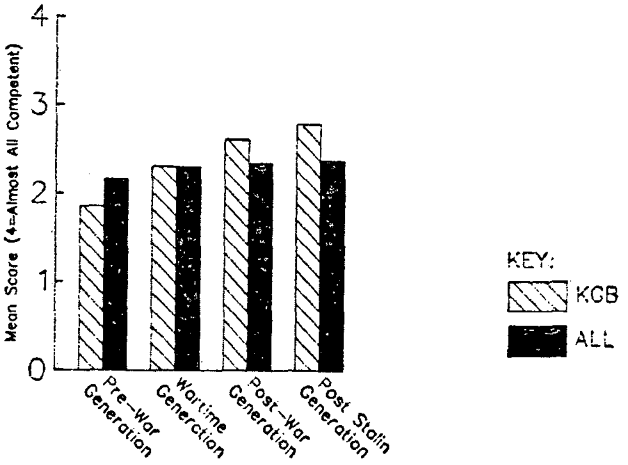
The results of a multiple regression analysis assessing the effects of all of these factors are shown in Table 3. The age categories are those used by Bahry (1987), and are based on Hough's (1980) classification of Soviet political generations. As expected, people's satisfaction with their material welfare is positively related to

Figure 5. Perceived Competence of Leaders of the KGB and All Major Institutions, by Generation within Levels of Education



Less Than Secondary Education

Complete Secondary Education



Complete Higher Education

their evaluation of the competence of the leaders of key Soviet institutions. In addition, the stronger people's support for regime norms, the more positive their image of institutional leaders, over and above the effects due to their satisfaction with material conditions. As shown in column 2, this relation holds up even after adjusting for the effects of the other factors.

Neither interest in politics, the number of privileges the respondent enjoyed, nor whether the respondent held a Party *nomenklatura* post[19] showed a statistically significant relation to the evaluation of the institutional leaders.

Consistent with the data in Figure 4, persons with higher education held Soviet authorities in lower esteem than those with less education, even after other factors are taken into account. Consistent with the results in Figure 5, respondents in the postwar generation (born 1926–40) and the post-Stalin generation (born 1941–60) held a more positive image of the competence of Soviet authorities than did earlier generations.

The comparatively positive assessment of the competence of the authorities by the younger generations and the comparatively negative assessment by the older generations thus persist even after a large number of additional respondent characteristics are taken into account. It seems likely that this pattern of evaluations is a generational rather than an aging phenomenon. That is, it reflects the historical experiences of differing cohorts, not a typical pattern of change in political beliefs through the life cycle. One indication is that the cohort differences are especially sharp in the evaluation of the competence and honesty of the most politically coercive agency, the KGB. The prewar generation (born before 1919, reaching age 18 before 1937), whose formative political experiences occurred during the Great Terror, has a particularly negative evaluation of the leaders of the KGB.

Table 3 also shows whether, after adjusting for other factors, people's direct experiences of being punished by the KGB affect their evaluations of the authorities. Respondents who were in families where another member was arrested for political reasons had a lower evaluation of the competence of officials. But those who themselves had previous punitive contact with the KGB (prior to the LNP) were not affected by this in their evaluation of the authorities (after adjusting for the effects of other factors).

In the latter case, it may be that most of the effect of people's experience with the KGB is captured by the measure of their subjective satisfaction. This is supported by additional analysis (not shown). Individuals who had been sanctioned by the KGB were much less satisfied with their material status in the LNP than those who did not have those experiences. Hence, via that mechanism, they had a lower estimation of the competence of the authorities in general.[20]

How do subjective satisfaction and social background affect evaluations of the KGB in particular? To answer this question, one must take into account that over half (56%) of the evaluation of the competence of leaders of the KGB can be accounted for by people's evaluations of the competence of leaders of the other seven institutions.[21] This means that if ratings of the KGB's competence were used directly as dependent variables in the equations for Table 3, the factors affecting evaluations of the KGB would work in much the same way as the factors affecting the average evaluation of all institutions. But the similarity in the results would be an artifact of the respondents' evaluation of the competence of institutional leaders in general.

To obtain a measure of the evaluation of the KGB that is distinct from the eval-

Table 3. Regression of Average Competency Ratings on Subjective Material Satisfaction, Support for Regime Norms, Measures of Status, and Experience of Direct Coercion

	Average Competency[a]	
Independent Variable	b (1)	b (2)
Subjective material satisfaction[b]		
Average satisfaction	199**	202**
Support of regime norms[c]		
State control	.032**	078**
Collective control	.082**	.037**
Interest in politics[d]		
Very interested		-.079
Somewhat interested		-.065
Slightly interested		-.054
Status measures		
Number of material privileges[e]		-.055
On Party *nomenklatura*[f]		-.097
Highest education[g]		
Complete secondary		-.064
Complete higher		-.095*
Generations[h]		
Wartime		077
Postwar		195**
Post-Stalin		.317**
Punitive experience before LNP[i]		
Punished by KGB		.045
Family member arrested		-.087*
Constant	2.014**	1.951**
Adjusted R²	.111	.129
Number of cases	2,167	2,207

[a] A single asterisk (*) indicates that the coefficient is significant at $p = .050$ using a one-tailed test. A double asterisk (**) indicates coefficient significant at $p = .050$ using a two-tailed test.

[b] *Subjective material satisfaction* is a summated scale based on responses to questions concerning the respondent's satisfaction with five material conditions of life during his last normal period of life (LNP) in the USSR. *housing, job, standard of living, medical care,* and *availability of goods.* The scale ranges between 2 (most satisfied) and -2 (least satisfied), with 0 as a midpoint. For further details, see Silver 1987.

[c] Scores on the *state-private control* measure and the *collective-individual* measure range from 7 (greatest state-collective control) to 1 (greatest private-individual control). Each measure is based on responses to three questions. For details, see Silver 1987.

[d] Dummy variables are used to represent the respondents' stated degree of interest in politics. The question was "During your last normal period [of life in the Soviet Union], how interested were you in politics and public affairs—were you *very interested, somewhat interested, only slightly interested,* or *not at all interested?*" The omitted category is those who said they were *not at all interested.*

[e] This is a count of the number of privileges the respondent claimed to have received during the LNP, from a list of three: *access to a closed medical clinic, access to special shops,* and *use of an official car.* Scores range from 0 to 3.

[f] Dummy variable: 1 if the respondent held a job controlled by the Party *nomenklatura,* 0 if not.

[g] Dummy variables representing highest diploma or degree earned. The omitted category is *less than complete secondary* education

[h] Dummy variables: *wartime,* born 1919–1925 (reached age 18 in 1937–1943); *postwar,* born 1926–1940 (reached age 18 in 1944–1958); *post-Stalin,* born 1941–1960 (reached age 18 in 1959–1978). The omitted category is persons born before 1919.

[i] Dummy variables: Did the respondent have an encounter with the KGB that led to some kind of negative sanction *before the beginning of the LNP?* Was any family member arrested for political reasons at some time prior to any arrest the respondent may have experienced, or prior to the LNP?

uation of the other agencies, we calculated a *KGB differential:* the difference between the respondents' actual (reported) evaluation of the competence of KGB leaders and their expected evaluation of KGB leaders given their evaluation of the competence of the leaders of the other seven institutions.[22] Positive KGB differentials mean that the respondents rated the KGB as more competent than would be expected from their rating of other authorities; negative KGB differentials mean that the respondents rated the KGB as less competent than would be expected.

Table 4 replicates the analysis in Table 3, but with the KGB differential for competence as the dependent variable. The question answered by this part of the analysis is, Do the explanatory variables have a distinctive effect on the evaluation of the competence of the KGB's leaders over and above their effect on the average evaluation of the leaders of all institutions? The answer depends on the variable considered.

Table 4. Regression of Differences between Actual and Expected Ratings of KGB Competence on Material Satisfaction, Support for Regime Norms, and Measures of Status and Experience of Direct Coercion

Independent Variable	Rating of Competence of KGB Minus Expected Rating[a] b (1)	Rating of Competence of KGB Minus Expected Rating[a] b (2)
Subjective material satisfaction		
Average satisfaction	−.057**	−.010
Support of regime norms		
State control	−.053**	−.036**
Collective control	−.031**	−.015
Interest in politics		
Very interested		.156**
Somewhat interested		−.031
Slightly interested		−.029
Status measures		
Number of material privileges		.025
On Party *nomenklatura*		−.078
Highest education		
Complete secondary		.025
Complete higher		.051
Generations		
Wartime		−.066
Postwar		.130*
Post-Stalin		.264**
Punitive experience before LNP		
Punished by KGB		.035
Family member arrested		.146**
Constant	.308**	−.095
Adjusted R^2	.024	.067
Number of cases	1,607	1,607

[a] A single asterisk (*) indicates that the coefficient is significant at $p = .050$ using a one-tailed test. A double asterisk (**) indicates coefficient significant at $p = .050$ using a two-tailed test.

One finding is that variation in people's subjective material satisfaction does not affect the size of the KGB differential. The KGB neither derives any distinctive benefit from increases in material satisfaction nor suffers any distinctive loss in reputation from decreases in people's material satisfaction.

In contrast, support for regime norms, particularly the norm of state control, is related differently to the evaluation of the KGB than it is to the evaluation of other institutional leaders. Persons more in favor of state control of the economy (state ownership of industry, state control of agriculture, state provision of medical care) give lower ratings to the KGB than to other institutional leaders, even after adjusting for the effects of age or generational differences.

Given that a high estimate of the competence of Soviet leaders is correlated with greater support for state control of the economy (see Table 3), the results in Table 4 can also be interpreted as showing that persons who have relatively positive evaluations of institutional leaders *other than the KGB* are more likely to favor state control. Additional statistical tests (not shown) sustain this interpretation: support for regime norms is directly related to the evaluation of the competence and honesty of institutional leaders as a whole (excluding the KGB) but inversely related to evaluations of the competence of the KGB in particular. Hence, the more competent or intimidating the KGB appears, the lower the support for regime norms.[23]

Also noteworthy are the effects of early punitive experience with the KGB. Respondents who were punished for unorthodox political activity did not have a measurably higher or lower estimate of the KGB's competence than those who were not punished. But those who had family members arrested had higher estimates of the KGB's competence (net of their estimate of the competence of other institutions) than those who did not.

These results do not present strong evidence that punitive contact with the KGB affects people's evaluation of the KGB. However, it should be recalled that there is a link between people's punitive experience and their subjective material satisfaction. Those who were punished for political reasons were less satisfied with their material welfare than those who were not, and lower material satisfaction is linked to more negative evaluations of the authorities in general.

Differences by generation also stand out in Table 4. Once again, the KGB receives a much higher rating on both competence and honesty by the postwar generation and the post-Stalin generation than by the prewar and wartime (born 1919–25) generations. It is especially notable that the generational differences in the evaluation of the competence of the KGB are so sharp even after we control for the effects of generalized attitudes towards institutional leaders and of several other factors. Not only are the younger generations likely to rate the authorities in general more highly than the older generations, but they are especially likely to rate the KGB more highly.

These results are consistent with an earlier finding of a lack of historical sense among younger respondents (Bahry 1987). A significant minority of the young, particularly the less educated, were likely to report that the KGB was more powerful in Brezhnev's time than in Stalin's.

Avoiding Trouble with the KGB. The SIP general survey also included questions designed to tap perceptions of the randomness of the KGB's actions and the pervasiveness of its network of informers. The questions are given in the notes to Table 5. The answers are instructive.

Almost half of all respondents stated that it was "easy or very easy" to avoid

Table 5. How Easy Is It To Avoid Trouble with KGB? How Easy Is It to Tell Who Is an Informer? (by Generation within Levels of Education)

Generation within Levels of Education	Respondents Who Said It Was Very Difficult or Somewhat Difficult: Percentage	Respondents Who Said It Was Very Difficult or Somewhat Difficult: Base Number of Cases
To avoid trouble with KGB		
Less than complete secondary		
Prewar generation	60.8	120
Wartime generation	54.4	68
Postwar generation	58.6	99
Post-Stalin generation	56.5	23
Complete secondary		
Prewar generation	64.8	108
Wartime generation	61.6	99
Postwar generation	49.8	247
Post-Stalin generation	52.6	470
Complete higher		
Prewar generation	66.0	53
Wartime generation	51.1	45
Postwar generation	43.4	318
Post-Stalin generation	34.7	398
To tell who is an informer		
Less than complete secondary		
Prewar generation	81.1	122
Wartime generation	77.6	67
Postwar generation	79.8	94
Post-Stalin generation	78.6	28
Complete secondary		
Prewar generation	92.2	115
Wartime generation	87.9	107
Postwar generation	83.1	267
Post-Stalin generation	85.8	471
Complete higher		
Prewar generation	88.3	60
Wartime generation	94.6	56
Postwar generation	85.6	361
Post-Stalin generation	85.5	421

Note: The two questions that were asked were: (1) "Some people say that it is easy for most Soviet citizens to avoid trouble with the KGB. Others say it is difficult to avoid trouble with the KGB. What did you think during your last normal period [of life in the Soviet Union]—was it *very easy* to avoid trouble with the KGB, *fairly easy, fairly difficult*, or *very difficult*?" (2) "Some people say that it is easy to tell who is a KGB informer or undercover KGB agent among one's coworkers, fellow students, or neighbors. Others say it is very difficult to know for sure. During your last normal period, did you think it was *very easy* to tell, *somewhat easy, somewhat difficult*, or *very difficult*?"

trouble with the secret police, hence that the actions of the KGB were predictable. But a much larger proportion—close to 80%—felt that it was somewhat difficult or very difficult to tell who might be a KGB informer among co-workers, fellow students, or neighbors.

The answers to these two questions taken together suggest yet another split image, between an organization with eyes and ears everywhere and one that is predictable and thus avoidable. The gap is especially pronounced for the younger generations and the highly educated. As Table 5 shows, virtually all the respondents agreed that it was difficult to know who was an informer or agent. Even so, the college-educated among the postwar and post-Stalin generations stand out as much more confident about the possibility of staying out of political trouble.

Further analysis (not shown) reveals that the respondents' sense of being able to calculate the odds, to avoid the secret police, is correlated with a more general sense of trust in other people. The lack of avoidance ability may thus be part of a more general syndrome of social atomization. On the other hand, the capabilities of the KGB to enforce social orthodoxy seem to be captured in the notion of the KGB's competence, and that is what we rely on as a measure of general deterrence in the analysis below.

Social Atomization

To understand how the intimidation factor might work to deter unorthodox political behavior, it is just as important to examine perceptions of other people as it is to examine perceptions of the KGB. To measure atomization, we rely on two items in the SIP questionnaire taken from Morris Rosenberg's (1956, 1957) widely used "misanthropy," or *faith-in-people,* scale. These questions were asked of a random one-third of the respondents. We combine the answers to the items to derive our measure of *faith in people,* which ranges in value from zero (least faith in people) to three (most faith in people).[24]

Although we hypothesize that those less trusting in other people will be less likely to become involved in unorthodox political activity, faith in people should be positively related to both unconventional *and conventional* political activism. This would be consistent with the findings in Almond and Verba's (1963) Five Nation Study, in which faith in people was important in accounting for cross-national differences in people's sense of political competence. Thus, trust of others may be an important political resource that facilitates participation in both unconventional and conventional political activity.

Table 6 shows that, within political generations, there is very little difference in faith in people across educational levels. In contrast, generational differences are very sharp. Within each educational level, those who came of political age before or during World War II had much less faith in people than those who came of political age after the war.

The low correlation between education and faith in people among SIP respondents contrasts with Almond and Verba's (1963) finding that trust in others increased with education in the five nations that they studied. The pattern among SIP respondents indicates that social atomization in the Soviet Union is not confined to persons of a certain social status. Instead, the level of interpersonal trust varies much more systematically by generation than by social status.

The evidence suggests for the older generation something of a "burnt-finger-tips" phenomenon, a wariness about letting down one's guard; while the younger generations, which came of political age in a period when the overt use of terror was much less common, were less wary of trusting others. Thus, the atomization and isolation of people from one another

Table 6. Faith in People, by Education and Generation

Education and Generation	Mean	Standard Deviation	Number of Cases
Level of Education within generations			
Prewar			
Less than complete secondary	.91	1.11	53
Complete secondary	.81	1.06	52
Complete higher	.82	.83	22
Wartime			
Less than complete secondary	.96	1.11	29
Complete secondary	1.00	.99	45
Complete higher	.83	1.13	18
Postwar			
Less than complete secondary	1.46	1.14	58
Complete secondary	1.18	1.12	107
Complete higher	1.24	1.08	133
Post-Stalin			
Less than complete secondary	1.50	1.17	15
Complete secondary	1.22	1.13	187
Complete higher	1.31	1.84	147
Generation within levels of education			
Less than complete secondary			
Prewar	.91	1.11	53
Wartime	.96	1.11	29
Postwar	1.46	1.14	58
Post-Stalin	1.50	1.17	15
Complete secondary			
Prewar	.81	1.06	52
Wartime	1.00	.99	45
Postwar	1.18	1.12	107
Post-Stalin	1.22	1.13	187
Complete higher			
Prewar	.82	.83	22
Wartime	.83	1.13	18
Postwar	1.24	1.08	133
Post-Stalin	1.31	1.84	147

Note: The two items used to construct the measure were (1) "Some people we've talked to say that most people can be trusted. Others say you can't be too careful in your dealings with people. How did you feel about that during your last normal period [of life in the Soviet Union]?" Answers coded: *Most people can be trusted, You can't be too careful,* and *It depends* (if volunteered). (2) "Speaking generally, would you have said during your last normal period that most people are more inclined to help others, or more inclined to look out for themselves?" Answers coded: *more inclined to help others, more inclined to look out for themselves,* and *it depends* (if volunteered).

Scores were derived by assigning a 3 to the answers *Most people can be trusted* and *more inclined to help others,* 1.5 to *It Depends,* and 0 to *You can't be too careful* and *more inclined to look out for themselves,* then averaging the values of each item. If only one item was answered with a valid value, only that item is used to compute the score.

appears to have been more or less indelibly impressed on the older generations but not firmly impressed on the younger ones.

This interpretation is supported by other evidence in the SIP general survey. The respondents were asked whether during their last normal period of life in the USSR they could have criticized an official (*ofitsial'noe litso*): to *nobody, members of their family, friends, other relatives, coworkers or fellow students,* or *their boss or teacher.*[25] While few respondents of any generation said they could have criticized an official to their boss or teacher, among respondents in the prewar and wartime generations, 18% said they could not have talked to anyone about this issue, while only 9% of those in the postwar generation and 4% of those in the post-Stalin generation said they could not have talked to anyone about it.

Of those who felt that they could criticize an official to someone, 44% of those in the prewar and wartime generations would extend the circle of communication only as far as their immediate family; while 31% of those in the postwar generation and only 22% of those in the post-Stalin generation would limit the criticism to their immediate family. This generational pattern holds up even after adjusting for differences due to education.

The differences in the level of faith in people may explain in part why the younger generations are more inclined to become involved in unorthodox political activity than the older ones. Being able to count on the support of others or, at least, relative freedom from worry about their doing harm to you, is likely to facilitate greater political involvement.

The Intimidation Factor and Political Activism

Our aim in the last part of this study is to determine whether intimidation served as a general deterrent on nonconformist behavior. We also explore the impact of intimidation on *compliant* political activism, to determine whether social atomization or fear of the authorities causes people to be more active in conventional participation and whether some of the sources of nonconformist activity also give impetus to compliant acts.

Measuring Activism. To measure the two dimensions of political activism, we constructed scales that capture both the level and intensity of each person's involvement.[26] For compliant activism we devised a summated scale based on the number of types of communal groups to which the respondent belonged in the LNP: housing, repair, or sanitary commissions; commissions at work or in local soviet or Party organizations; membership in the citizens' militia (*druzhina*) or comrades' courts; parents' commissions; the Komsomol; and other communal organizations.[27]

This measure is weighted by the intensity of involvement in each type of group —whether the respondent was a leader, attended regularly, attended occasionally, or belonged but never attended.[28] About half the respondents engaged in some such activity; the average score for the respondents as a whole is 2.1 out of a possible 28, with individual scores ranging from 0 to 28 and a standard deviation of 2.96.

Our measure of nonconformist activism follows the same logic. It is constructed by totaling the number of different types of nonconformist activity in which the respondent engaged during the LNP, from unsanctioned study groups to distributing *samizdat,* engaging in protests, and strikes. It also includes a count of other unconventional acts as defined by the respondent; and it counts similar behaviors reported in response to questions about contacts with the KGB during the respondent's LNP. Ten percent of all respondents participated in some form of nonconformist activity as defined here; and 2% more were leaders. Although actual individual scores range from 0 to 16, the average score for the respondents as a whole is very low—.41, with a standard deviation of 1.44.[29]

Hypotheses. We conceptualize the impact of intimidation—fear of the KGB and of other people—on unconventional political behavior by regarding nonconformity as a result of three types of variables: (1) those that *motivate* the expression of political disaffection, (2) those that *facilitate* its public manifestation, and (3) those that *inhibit* its manifestation. This approach is similar to that proposed by Gurr (1968) for explaining civil strife. Gurr's model posits that there are sources of dissatisfaction (relative deprivation) that motivate people to engage in civil strife, but whether or not this engagement occurs is mediated by the factors of state coercion, legitimacy, institutionalization, and facilitation.

As indicators of the motivation for expressing disaffection, we include the respondents' satisfaction with their material quality of life during their LNP, and the respondents' support for established regime norms. Unconventional activists should be less satisfied with their material quality of life, established regime norms, or both.

Certain political resources should facilitate nonconformity. Education, for example, should provide the conceptual skills necessary to visualize alternatives and to manipulate the system (Moore 1954). Increasing education has been shown already to be related to decreasing support among SIP respondents for established institutional practices (Silver 1987). Interest in politics can also be viewed as a resource, one that prompts the individual to seek out more information and thus to weigh the regime's message against other (foreign and domestic) sources. Interest can thus help to reduce the ambiguity surrounding public issues, and the less the ambiguity, the greater the likelihood of questioning the established order (Jahoda and Cook 1954).

Earlier research has shown that the generations that came of age after Stalin were both the ones most likely to engage in compliant activity and the ones most likely to engage in nonconformist activity (Bahry 1987). The young were also more mobilized in the conventional sense (Zimmerman 1987). These higher levels of both types of activism are a product of the renewal of mass organizations after Stalin and of changes in political atmosphere that prompted the growth of dissident movements after Khrushchev. We should expect to find similar generational differences in the level of political involvement in the analysis here.

A person's status may be important as well: those with more material privileges and a job on the Party's *nomenklatura* are likely to have connections and thus a degree of insulation from reprisals, at least for the more common brands of unconventional behavior. On the other hand, persons whose jobs are controlled by the Party's *nomenklatura* may also be more immediately subject to discipline for transgressions against official standards of behavior.

The intimidation factor should tend to inhibit overt unconventional activity. Individuals with a high regard for the capabilities of the secret police and those with low faith in other people should be less likely than others of the same age, education, and degree of disaffection to overstep the bounds of acceptable political behavior.[30]

Nonconformist Activism. Column 1 in Table 7 shows the relation between nonconformist activism and the hypothesized motivational factors.[31] As expected, people less supportive of regime norms (particularly state control of the economy) were more likely to be nonconformists.

Table 7. Regression of Nonconformist and Compliant Activism on Subjective Satisfaction, Support for Regime Norms, Atomization, and Perception of Coercion

	Nonconformist Activism			Compliant Activism
Independent Variable	b (1)	b (2)	b (3)	b (4)
Subjective material satisfaction				
Average satisfaction	-.087**	-.095**	-.066**	-.026
Support of regime norms[a]				
State control	-.225**	-.233**	-.163**	.061
Collective control	-.008	-.009	.039	.154**
Atomization-coercion				
Faith in people[b]		.062**	.039**	.060*
KGB highly competent[d]		-.039	-.067*	-.011
Interest in politics				
Very interested			.214**	.221*
Somewhat interested			-.005	.112
Slightly interested			-.016	.063
Status measures				
Number of material privileges			.140**	.165**
On party *nomenklatura*			-.178**	.510**
Highest education				
Complete secondary			-.057	.043
Complete higher			.053	.204*
Generations				
Wartime			.044	.200
Postwar			.110*	.306**
Post-Stalin			.196**	.651**
Punitive experience before LNP				
Punished by KGB			.301**	-.436*
Family member arrested			.065	-.064
Constant	.479**	.439**	.157	-.119
Adjusted R^2	.075	.090	.179	.146
Number of cases	612	612	612	612

Note: Logarithms of the dependent variables are used. A single asterisk (*) indicates that the coefficient is significant at $p = .050$ using a one-tailed test. A double asterisk (**) indicates coefficient significant at $p = .050$ using a two-tailed test.

[a] The natural log logarithm of the values of state-private and collective-individual control was used in the equations for this table.

[b] Scores range from 3 (most faith in people) to 0 (least faith in people).

[c] Dummy variable: 1 if respondent said that *most* or *almost all* of the leaders of the KGB were competent, 0 if not.

Columns 2 and 3 add in the effects of atomization, coercion, and the other variables hypothesized to facilitate or inhibit the manifestation of nonconformity. As the atomization thesis would predict, the greater people's interpersonal trust (faith in people), the more likely they were to be nonconformist. Similarly, persons who perceived the KGB to be highly competent were less likely to engage in unorthodox activity. Thus, both components of the intimidation factor appear to operate in the expected direction: social atomization and perceptions of the KGB's competence serve to deter would-be nonconformists. At the same time, consistent with our earlier findings concerning *individual* deterrence, prior punishment by the KGB is associated with an increased likelihood that the respondents were nonconformist in their LNP.

Persons who were more interested in political affairs were more likely to be unconventional activists, even after adjusting for the effects of education, material satisfaction, and support for the regime. At the same time, interest in politics is also positively associated with *compliant* activism (see column 4). Thus, those who are interested in politics are likely to be motivated to some sort of action whatever their political orientation.

Having more material privileges is also positively related to nonconformity. One explanation for this may be that persons with access to privileges benefit from greater *protektsiia* (connections) with people in high places, and hence risk less by their involvement in nonconformist activities.[32] Another explanation may be that the privileged among the SIP respondents more often were in the arts and culture and not only had more temptations and opportunities for engaging in unconventional activities but also were more attracted to them. A third explanation may be that having access to privileges is linked to greater incentives to become involved in political affairs, both nonconformist and conformist. That there is a positive relation between access to privileges and participation in compliant activism (column 4) is consistent with this interpretation.

Persons who held important official positions that require Party approval were less likely to be involved in nonconformist activity and more likely to be compliant activists than those who did not hold such positions (compare columns 3 and 4). Thus, they may have been inhibited by Party control from engaging in unconventional behavior, whatever their personal sense of material satisfaction or level of support for regime norms.

As expected, persons in the postwar and post-Stalin generations were more involved in political nonconformity, even after we adjust for their more critical views of established institutional practices, their lower levels of satisfaction (higher aspirations) concerning material conditions, their greater sense of the capabilities of the leaders of the KGB, and their higher levels of interpersonal trust. In other words, the relation between generation and nonconformist activism holds up even after adjusting for the effects of motivational factors, sense of intimidation, and degree of faith in people.

One could, of course, maintain that young people are inclined toward nonconformity, and that progression through the life cycle fosters greater compliance as people develop a greater stake in society in connection with forming their own family and developing their careers. Evidence in the survey challenges the life-cycle interpretation, however. Nonconformity did not diminish evenly with age among the postwar and post-Stalin generations (see Bahry 1987). It is also important to keep in mind that the "younger generations" in the survey do not consist of "very young people." Respondents in the postwar generation had a median age of 41 at the midpoint of their LNP; and respondents in the post-Stalin generation had a median age of 28 at the midpoint of their LNP.

The younger generations were also more likely than the older generations to be involved in *compliant* political organizations. Hence, the activism of the younger cohorts appears to reflect special formative experiences and commitments of those generations that cannot be readily reduced to a set of explicitly measured motivational and ideological factors.

Compliant Activism. The results in column 4 indicate that the motivation for compliant activism derives from something other than dissatisfaction with the material quality of life. Nor is it an expression of satisfaction with the established political order. That those with higher education, as well as those who hold Party *nomenklatura* positions, were more likely to be compliant activists implies that they were responding to the expectation for persons of their status to become involved in public organizations and *obshchestvennaia rabota* (work for society). Hence, such activism may be less a reflection of individual conviction and more a calculated conformity, a formal adherence to official norms of behavior intended to assure physical survival or to secure career advancement (for a similar interpretation, see Shtromas 1984).

Comparison of the determinants of nonconformist and compliant activism reveals that some factors both motivate and give a direction to political activism —toward conformity or nonconformity. These include degree of satisfaction with the material quality of life, level of support for regime norms, and whether a person holds a job that is under the control of the Party's *nomenklatura.* Other factors stimulate involvement in both types of activism: faith in people, interest in politics, access to privilege, and coming of political age in the postwar period.

Intimidation, however, works differently with respect to the two types of political involvement. A perception of the KGB as competent inhibits nonconformity but does not breed greater compliant activism, while atomization appears to deter both types of political activism, nonconformist and compliant.

Conclusions and Implications

Our analysis confirms that general deterrence works, at least to a degree. What is striking is that individual deterrence does not. Perceptions of the regime's coercive capability tend to inhibit dissent *ex ante;* but once people do transgress the borders of acceptable political behavior, punishment (short of incapacitation) is not likely to keep them from further nonconformity. We think this indicates that the experience of being punished reduces an individual's possible gains from future compliance and induces a more permanent hostility to the regime.

Thus, there appears to be a trade-off between individual and general deterrence. By cutting off the return route to normalcy for those whom it punishes, the state assures a certain amount of recidivism and may create a class of people who are permanently alienated. At the same time, the unidirectional nature of this route heightens the effectiveness of *general* deterrence: the permanency of the stigma of being punished may be a key to deterring the otherwise disaffected from nonconformity in the first place.

Therefore, aside from the question of cost-effectiveness, singling out leaders for reprisals is logical from the regime's standpoint. By not trying to punish every transgressor of official norms, the state minimizes the number of people permanently alienated by being penalized. But punishing leaders provides a demonstration effect that enhances general deterrence.

Our data also reveal that the modernization of coercion has engendered a sense of its predictability. Soviet citizens, especially the young and the highly educated, seem to believe they know the odds of reprisal if they violate official political norms. However, the differences in such perceptions suggest that the reduction in the use of mass terror since Stalin's day has had different impacts on different publics.

For witnesses of the Great Terror in the late 1930s, the message is still clear: *the KGB can still get you, and you shouldn't trust other people.* Those who survived the Stalin era see the secret police as less competent, perhaps because of the arbitrariness and sheer scope of police activity during that time. But among these generations, the more competent the KGB *is* perceived to be, the less nonconformity.

For the young, the Great Terror is an appropriated memory. The postwar and post-Stalin generations view the KGB as more professionally competent, but they are also more inclined to see the KGB's actions as predictable and avoidable. Without the experience of the Stalin era, they seem to feel better able to calculate the odds of reprisal. Equally important, these generations have grown up in an atmosphere of more stable social relations. Mistrust of others has diminished; these generations are less atomized.

Not all members of the postwar and post-Stalin generations see the world through the same eyes, however. The college-educated are much more sanguine about their ability to avoid political trouble, in part because they possess the conceptual skills and information needed to manipulate the system, and in part because the kinds of unconventional activities in which they are most likely to engage bring fewer reprisals. Few unsanctioned "intellectual" activities (e.g., unsanctioned study groups, reading and distributing *samizdat-tamizdat*) evoked any official penalty. But the types of nonconformity in which the *less* educated are likely to engage (e.g., strikes) are much more likely to evoke a coercive response.

The regime's selectivity in dealing with unorthodox behavior thus carries class overtones and may help to explain why there are fewer blue-collar dissenters. In addition, the less educated are more satisfied with material conditions, more supportive of state controls over the economy and over the citizenry (Silver 1987), and more positive in their view of the leaders of Soviet institutions in general.

For Soviet leaders, there appears to be a trade-off between building up mass support for the established political order and building up the image of the KGB. The greater the sense of material satisfaction, then the more positive the evaluation of Soviet leaders as a whole, and the stronger the backing for the established norms of state control. But the greater the perceived capability of the KGB—that is, the more coercive the regime is believed to be—the weaker the popular support for the regime.

Our results thus suggest that if Soviet leaders were to downgrade the KGB, they could increase support for the regime. But a KGB with a less potent image would also be less able to deter nonconformity. Hence, a leadership that considers the maintenance of its own control over society to be a paramount goal is likely to continue to promote the KGB's image. As long as the KGB does not threaten the leadership itself, the leadership can accept the resulting reduction in popular support as a necessary cost.

In sum, the regime's ability to maintain social control relies on a correlation of forces: the manipulation of material and normative rewards and a mix of mistrust and intimidation. One critical variable is the perceived level of material welfare of the population. The greater the sense of material well-being, the greater the popular support for Soviet political orthodoxy and the more positive the evaluations of Soviet leaders. But even among those who have a low sense of material satisfaction and low support for the system, social atomization and perceptions of the regime's coercive potential still inhibit nonconformity.

Our data show that the balance is changing, however. The success of Soviet leaders in maintaining political peace depends on their striking a balance between individual and general deterrence, on a residuum of social atomization, and on a reserve of popular support that is stronger among the older generations and the less educated. But with the succession of generations, the social atomization bred by the Great Terror may be withering away, along with its inhibiting effect on unconventional behavior. Generational replacement and increasing educational attainments also appear to be weakening support for the old orthodoxies. Therefore, a regime bent on suppressing dissent and open opposition must rely on other means to achieve the same degree of control.

Appendix

Table A-1. Mean Rating of Leaders of Institutions for Competence and Honesty, by Level of Education within Generations

Level of Education Within Generations	Local Party	Local Soviet	Local Police	Managers of Industry	Academy of Sciences	Military	Politburo	KGB	All
COMPETENCE									
All respondents									
Less than secondary	2.29	2.40	2.30	2.65	2.91	2.94	2.36	2.45	2.54
Complete secondary	2.13	2.20	2.20	2.58	2.86	2.90	2.33	2.62	2.48
Complete higher	1.94	2.01	2.10	2.46	2.77	2.72	2.07	2.64	2.34
Prewar									
Less than secondary	2.17	2.22	2.09	2.55	2.81	2.74	2.18	2.43	2.40
Complete secondary	1.97	2.16	2.03	2.45	2.72	2.72	2.04	2.03	2.26
Complete higher	1.67	2.00	1.92	2.43	2.51	2.59	1.73	1.85	2.16
Wartime									
Less than secondary	2.12	2.40	2.44	2.63	2.77	2.98	2.17	2.39	2.53
Complete secondary	1.95	2.17	2.08	2.48	2.70	2.68	2.11	2.13	2.32
Complete higher	1.93	1.98	1.91	2.56	2.70	2.74	1.97	2.30	2.30
Postwar									
Less than secondary	2.48	2.50	2.42	2.73	3.07	3.09	2.60	2.45	2.68
Complete secondary	2.17	2.24	2.20	2.53	2.76	2.92	2.37	2.61	2.47
Complete higher	1.97	2.00	2.13	2.45	2.76	2.66	2.02	2.61	2.34
Post-Stalin									
Less than secondary	2.33	2.77	2.46	2.78	3.00	2.96	2.57	2.67	2.65
Complete secondary	2.18	2.20	2.25	2.65	2.96	2.96	2.39	2.81	2.56
Complete higher	1.94	2.02	2.11	2.46	2.82	2.78	2.15	2.78	2.37
HONESTY									
All respondents									
Less than secondary	1.76	1.88	1.69	2.02	2.34	2.53	1.76	1.49	1.99
Complete secondary	1.48	1.61	1.40	1.86	2.20	2.20	1.36	1.24	1.70
Complete higher	1.10	1.37	1.21	1.84	2.08	1.94	.93	.94	1.46
Prewar generation									
Less than secondary	1.59	1.85	1.63	1.99	2.21	2.34	1.58	1.34	1.89
Complete secondary	1.59	1.79	1.59	1.90	2.35	2.29	1.41	1.10	1.80
Complete higher	1.31	1.63	1.29	1.93	2.09	2.08	1.00	.85	1.57
Wartime									
Less than secondary	1.55	1.69	1.45	1.78	2.04	2.44	1.47	1.31	1.84
Complete secondary	1.51	1.70	1.48	1.92	2.11	2.20	1.52	1.30	1.73
Complete higher	1.21	1.35	1.22	1.80	2.17	2.15	1.20	1.02	1.54
Postwar									
Less than secondary	2.06	2.04	1.88	2.18	2.58	2.75	2.07	1.65	2.18
Complete secondary	1.43	1.55	1.30	1.84	2.13	2.19	1.26	1.13	1.63
Complete higher	1.06	1.30	1.17	1.78	2.00	1.90	.88	.91	1.43
Post-Stalin									
Less than secondary	1.76	1.93	1.80	2.07	2.65	2.62	2.05	1.96	2.12
Complete secondary	1.48	1.59	1.40	1.86	2.22	2.19	1.38	1.32	1.71
Complete higher	1.09	1.39	1.24	1.88	2.14	1.94	.93	.95	1.46

Table A-2. Mean Rating of Leaders of Institutions for Competence and Honesty, by Generation within Levels of Education

Generation within Levels of Education	Local Party	Local Soviet	Local Police	Managers of Industry	Academy of Sciences	Military	Politburo	KGB	All
COMPETENCE									
All respondents									
Prewar	1.99	2.15	2.04	2.49	2.71	2.71	2.04	2.17	2.30
Wartime	2.00	2.19	2.14	2.54	2.72	2.78	2.10	2.24	2.38
Postwar	2.12	2.16	2.20	2.52	2.80	2.82	2.23	2.59	2.44
Post-Stalin	2.07	2.13	2.19	2.57	2.90	2.88	2.29	2.79	2.48
Less than secondary									
Prewar	2.17	2.22	2.09	2.55	2.81	2.74	2.18	2.43	2.40
Wartime	2.12	2.40	2.44	2.63	2.77	2.98	2.17	2.39	2.53
Postwar	2.48	2.50	2.42	2.73	3.07	3.09	2.60	2.45	2.68
Post-Stalin	2.33	2.77	2.46	2.78	3.00	2.96	2.57	2.67	2.65
Complete secondary									
Prewar	1.97	2.16	2.03	2.45	2.72	2.72	2.04	2.03	2.26
Wartime	1.95	2.17	2.08	2.48	2.70	2.68	2.11	2.13	2.32
Postwar	2.18	2.24	2.20	2.53	2.76	2.92	2.37	2.61	2.47
Post-Stalin	2.18	2.20	2.25	2.65	2.96	2.96	2.39	2.81	2.56
Complete higher									
Prewar	1.67	2.00	1.92	2.43	2.51	2.59	1.73	1.85	2.16
Wartime	1.93	1.98	1.91	2.56	2.70	2.74	1.97	2.30	2.30
Postwar	1.97	2.00	2.13	2.45	2.76	2.66	2.02	2.61	2.34
Post-Stalin	1.94	2.02	2.11	2.46	2.82	2.78	2.15	2.78	2.37
HONESTY									
All respondents									
Prewar	1.53	1.78	1.55	1.95	2.24	2.27	1.41	1.16	1.80
Wartime	1.44	1.61	1.41	1.85	2.11	2.26	1.43	1.24	1.72
Postwar	1.34	1.50	1.32	1.86	2.12	2.14	1.18	1.10	1.61
Post-Stalin	1.31	1.51	1.33	1.87	2.20	2.09	1.19	1.17	1.61
Less than secondary									
Prewar	1.59	1.85	1.63	1.99	2.21	2.34	1.58	1.34	1.89
Wartime	1.55	1.69	1.45	1.78	2.04	2.44	1.47	1.31	1.84
Postwar	2.06	2.04	1.88	2.18	2.58	2.75	2.07	1.65	2.18
Post-Stalin	1.76	1.93	1.80	2.07	2.65	2.62	2.05	1.96	2.12
Complete secondary									
Prewar	1.59	1.79	1.59	1.90	2.35	2.29	1.41	1.10	1.80
Wartime	1.51	1.70	1.48	1.93	2.11	2.20	1.52	1.30	1.73
Postwar	1.43	1.55	1.30	1.84	2.13	2.19	1.26	1.13	1.63
Post-Stalin	1.48	1.59	1.40	1.86	2.22	2.19	1.38	1.32	1.71
Complete higher									
Prewar	1.31	1.63	1.29	1.93	2.09	2.08	1.00	.85	1.57
Wartime	1.21	1.35	1.22	1.80	2.17	2.15	1.20	1.02	1.54
Postwar	1.06	1.30	1.17	1.78	2.00	1.90	.88	.91	1.43
Post-Stalin	1.09	1.39	1.24	1.88	2.14	1.94	.93	.95	1.46

Table A-3. Pearson Correlations between Evaluations of Competence and Honesty of Leaders of Eight Soviet Institutions

Institution	Local Soviet	Local Party	Local Police	Managers of Industry	Academy of Sciences	Military	Politburo	KGB
COMPETENCE								
Local Soviet	1.0	.68	.59	.65	.53	.57	.60	.42
Local Party		1.0	.66	.52	.47	.53	.74	.58
Local Police			1.0	.51	.48	.55	.65	.64
Managers of Industry				1.0	.60	.59	.50	.46
Academy of Sciences					1.0	.65	.50	.49
Military						1.0	.54	.57
Politburo							1.0	.66
KGB								1.0
Average R								.55
HONESTY								
Local Soviet	1.0	.79	.73	.65	.64	.66	.70	.66
Local Party		1.0	.76	.61	.60	.62	.80	.77
Local Police			1.0	.62	.58	.61	.71	.73
Managers of Industry				1.0	.64	.65	.54	.52
Academy of Sciences					1.0	.76	.56	.53
Military						1.0	.60	.56
Politburo							1.0	.84
KGB								1.0
Average R								.66

Notes

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1. We refer to *nonconformity* and *unconventional activity* rather than *dissent* for two reasons: First, the term *dissent* implies active, programmatic opposition, while we are concerned with the disposition to overstep the bounds of politically correct behavior, programmatic or not. Second, most respondents who did overstep would not necessarily refer to themselves as *dissidents*.

2. In two cases, part of the respondent's LNP occurred during Nikita Khrushchev's regime. In all other cases, the respondent's LNP both began and ended during Brezhnev's regime. In 92% of the cases, the respondent's LNP ended between 1978 and 1981, inclusive.

3. Since our concern is with adult political activity, we restrict our analysis to the 2,667 respondents who had reached age 18 at the start of their LNP. Field work for the survey was conducted by the National Opinion Research Center. The average length of the interviews was three hours. All but 10 of the interviews were conducted in Russian. The sample was a stratified random sample based on a list of adult emigrants from the USSR to the United States between the dates mentioned. Participation in the survey was voluntary. The response rate was 79%. For further information about the sample, see Anderson and Silver 1987a.

4. The respondent's ethnic identity comes into play in two ways. One is on questions asking specifically about ethnic relations and nationality policies in the USSR. Second, Soviet emigrants in our study came from two basic groups: persons in the third wave of the Jewish emigration (which includes many non-Jewish spouses and family members), and individuals who were political dissidents or other special cases involving family

reunification (this group consists primarily of non-Jews, but it also includes many Jews). For this reason, non-Jewish respondents were more likely to be politically nonconformist than Jewish respondents. For further discussion of potential sources of bias in the survey, see Bahry 1985, 1987; Silver 1987; Anderson and Silver 1987a, 1987b; Millar 1986; and Swafford et al. 1987.

5. These concepts are widely used in the literature on criminology. For a brief statement, see von Hirsch 1976.

6. *Samizdat* (literally "self-publication") is underground material by Soviet authors. *Tamizdat* (literally "publication over there") is material by Soviet authors that has not been approved for publication or distribution in the Soviet Union but has been published abroad.

7. Thomas Schelling has observed that threats can often be made more credible if the actor who makes the threat appears to be irrational and unpredictable: "The essence of randomization in a two-person zero-sum game is to preclude the adversary's gaining intelligence about one's own mode of play—to prevent his deductive anticipation of how one may make up one's own mind, and to protect oneself from tell-tale regularities of behavior that an adversary might discern or from inadvertent bias in one's choice that an adversary might anticipate" (1963, 175).

8. Many more engaged in "private" nonconformity such as reading of *samizdat-tamizdat*. And many engaged in behaviors that are officially frowned upon but tacitly allowed, such as listening to Western radio broadcasts. In addition, almost 20% of the respondents indicated that they had attended unofficial art shows or other cultural events; but information from interviewers suggests that many people did not hear the word *unofficial* in the question and hence answered in terms of the regime-sanctioned cultural activities that they attended. See Bahry 1987.

9. Punishment includes any action taken against the respondent by any agency or official (including the KGB, the Party, supervisors at work, and other organizations or individuals sanctioned by the state). The penalties include reprimands at work or school; interrogations or reprimands by Party officials or the KGB; being threatened, harassed, followed, or arrested. Most official actions taken against the individual were reprimands, warnings, or interrogations.

10. It is important to note that the boundaries of the permissible fluctuate over time (Reddaway 1983). But there appear to be clear patterns of reprisal for the period under study.

11. This information is obtained by asking questions about any official actions that were taken in connection with the respondents' participation in other unconventional activities, their failure to participate in mobilized participation (such as voting), or their making individual protests to officials, as well as the question, Other than what you may have already told me, did you have any (other) contact with the KGB?

12. The low frequency of contact with the KGB overall among SIP respondents could reflect a reluctance of the respondents to speak frankly about these contacts. However, the frequency of contact is consistent with Reddaway's (1983) estimates of some two hundred political arrests per year in the late 1970s in the entire USSR.

13. We also tested whether the effect of individual deterrence might depend on whether a person was arrested or interned rather than harassed or interrogated; but the severity of punishment made no difference in later political nonconformity. To be complete, the analysis should also include data on the incidence of recidivism among people who were nonconformists before their LNP but who were not penalized for it, but the survey does not provide this information.

14. This contrasts with the general Soviet philosophy of punishment, which focuses on rehabilitation rather than retribution (Hazard 1983, 99ff).

15. Two series of questions were asked. The first concerned the honesty of officials: "Here is a list of some institutions that exist in the Soviet Union. I would like you to think about how many of the people in charge of each of them you felt were *honest* during your last normal period. For each one, circle the number under the category that best describes how many of the people in charge were *honest*" (*almost all, most, some, hardly any*, or *none*).

The second series concerned the competency of officials: "Now turn the page over. On this side, think about how many of the people in charge at each of these institutions were competent, that is, did their jobs well. Remember, try to think about how you felt during your last normal period."

"People in charge" translates as *rukovoditeli;* "competent" translates as *kompetentnyi;* and "honest" translates as *chestnyi. Chestnyi* may be interpreted not simply as "honest" but also as "honorable" or "having integrity."

16. A factor analysis of the responses to the 16 items confirms that the responses to the competence items are distinct from the responses to the honesty items. The analytic results are not presented here.

17. *None* is assigned a value of 0; *hardly any*, 1; *some*, 2; *most*, 3; and *almost all*, 4. Averaging the evaluations across all eight institutions, we get a mean value on competence of 2.43, with a standard deviation of .782.

18. The "state-control" dimension is reflected in answers to questions about the respondent's preference for state ownership of heavy industry, state control of agriculture, and state provision of medical care. The "collective-control" dimension is reflected in answers to questions about limitations on the right to strike, control of population movement

through residence permits, and protecting the rights of society rather than the rights of individuals accused of crimes. See Silver 1987.

19. A Party *nomenklatura* post is a job or position in the state or cooperative sector for which a Party official controls personnel selection. The official can either directly determine or exercise a veto over who fills the post. Five percent ($N = 134$) of the respondents reported that their job during their LNP was a Party *nomenklatura* position.

20. Full specification of the relationships between experience with the KGB, subjective material satisfaction, and evaluation of the authorities requires a multiequation model. Our main concern here is whether the relationship between the respondents' evaluations of the authorities and their social backgrounds, subjective material satisfaction, and support for regime norms remains even after their punitive experiences are taken into account. The answer is *yes*, they do.

21. This is the r-square obtained by regressing the evaluation of the competency of the leaders of the KGB on the evaluations of the competency of the other institutional leaders.

22. This involves the use of an instrumental variable derived by regressing the evaluation of the KGB on the evaluations of the other seven agencies. The resulting regression coefficients were then used to estimate an expected evaluation of the KGB for each respondent based on the respondent's evaluation of the other agencies. The expected KGB evaluation was then subtracted from the respondent's actual (reported) KGB evaluation to derive a *KGB differential*—the difference between the actual and expected rating of the KGB's competence given the respondent's ratings of the other institutions. The mean value for the KGB differential is 0, with a standard deviation of .781, and with values ranging between −3.3 and 3.5.

23. This can be shown by reversing the assumed direction of the causal relationship between support for regime norms and the evaluation of competence of the authorities from the direction specified in Tables 3 and 4. However, to study individual differences in the evaluation of the authorities, it seemed most reasonable to assume that these evaluations are contingent on the individual's subjective material welfare and on his or her affect toward the regime in general. The relation could in fact be reciprocal.

24. The mean on the faith-in-people measure is 1.172, the standard deviation is 1.117, and scores range from 0 to 3. The wording of the items and the method of calculating the combined measure is given in the notes to Table 6. Three additional items from Rosenberg's misanthropy scale were included in the survey. These were agree-disagree items rather than balanced items with arguments and counterarguments; they appear to have been subject to a substantial amount of acquiescence response bias. We exclude them from the analysis for this reason.

25. This was one of a series of questions: "We are interested in the kinds of things people in the Soviet Union discuss with one another. Please look at this card, and tell me with which of these people you could have [*vy mogli by*] talked to during your last normal period about . . . the latest fashions or sports event? where to buy clothing on the black market? criticizing a government official?" The card listed the five groups mentioned in the text, and multiple answers were recorded for each question.

26. We performed a similar analysis of the impact of intimidation on individual contacting of government and party organs and on the use of *blat* or *protektsiia* (connections) in such bureaucratic encounters. The results suggest that the variables we examine here (in particular, perceptions of the competence of the KGB and of interpersonal trust) have little effect on such behaviors. Contacting involves a distinct type of activity with different costs and benefits that mobilize the public in a different way.

27. We counted *leadership* in a trade union as a conventional activity; but simple membership was excluded since it is all but mandatory. Party membership could not be included: in pretests of the questionnaire, a direct question about whether respondents belonged to the Party proved to be so sensitive that it was omitted from the survey. However, our measure of whether the respondents held Party *nomenklatura* posts in the LNP is a partial substitute for such a variable.

28. For each type of activity during the LNP, respondents were given one point if they belonged but never attended meetings, two points if they attended sometimes, three points if they attended regularly, and four points if they were leaders. For Komsomol members, the questionnaire asked only whether the respondents had ever belonged and whether they had been leaders. We counted only those leaders and members who had been in the Komsomol during their LNP.

29. The summary measure assigns two points for each type of nonconformist activity in which the respondent participated (but did not lead) during the LNP and four points for each type of activity in which the respondent took a leading role.

30. Our measure of intimidation is a binary variable derived from the question on competence of the KGB. Respondents who reported that *most* or *almost all* of the leaders of the KGB were competent are assigned a value of one; respondents who stated that *some*, *hardly any*, or *none* of the leaders of the KGB were competent are assigned a value of zero. Responses coded as *don't know*, *not ascertained*, and *refused to answer* are coded as missing.

31. A majority of the cases are scored as zero on each dependent variable. This suggests that it would be appropriate to use a method other than ordinary-least-squares (OLS) regression to test for the sources of compliant and nonconformist activism. In

analyses not shown here, we applied probit analysis to test the same hypotheses. We tested first whether people engaged in *any* kind of nonconformist political behavior and then whether those who engaged in any such behavior were likely to become leaders. The results suggest that the independent variables play different roles in accounting for transitions across different thresholds of nonconformity. Although there are some differences in the relative strengths of the independent variables when this alternative technique is used instead of OLS, the basic conclusions suggested by the probit analysis are similar to those produced by OLS.

32. However, we tested whether political nonconformists were less likely to be punished if they held a Party *nomenklatura* post or were compliant activists. The answer is *no:* the incidence of punishment depended more on the type of nonconformity than on job status or participation in approved activities.

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HOW ARE FOREIGN POLICY ATTITUDES STRUCTURED? A HIERARCHICAL MODEL

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It has long been assumed that foreign-policy attitudes of the mass public are random, disorganized, and unconstrained if they exist at all. Further, foreign-policy thinking has not been found to be structured along standard ideological (liberal-conservative) lines, partisan lines, or class lines. We attempt to move the discussion from a question of whether *foreign-policy attitudes are structured to a question of* how *they are structured. We propose and estimate (using a LISREL model) a hierarchically organized foreign-policy belief structure in which specific policy preferences are derived from* postures *(broad, abstract beliefs regarding appropriate general governmental strategies). These postures, in turn, are assumed to be constrained by a set of core values about the international community.*

It is by now accepted as a truism that structure and consistency are highly desirable properties of a political belief system—carrying important implications for the competence of the average citizen as well as the functioning of the democratic process. At the individual level, citizens with more consistent and highly related attitudes are assumed to be more thoughtful and efficient in their political reasoning. And at the system level, communication between political elites and the mass public is assumed to be greatly facilitated when the electorate imposes a common and meaningful structure on its attitudes.

Because of the importance of the topic, the past two decades have produced numerous studies investigating the degree to which citizens demonstrate any meaningful structure in their political attitudes (see Asher 1984; Kinder 1983; and Niemi and Weisberg 1984 for chronicals). The seminal research was conducted by Converse (1964), who found very little consistency either among domestic attitudes, among foreign-policy attitudes, or between the two policy domains. As one recent reviewer of the debate has concluded, if we take ideological constraint to mean that specific policy positions are deduced from widely shared, sweeping ideological principles, then the consensus of research clearly indicates that most U.S. citizens are "innocent of ideology" (Kinder 1983, 401). For example, it is evident that specific issue positions in such diverse domains as social-moral policy, economic policy, and foreign policy do not derive from the single liberal-conservative dimension for which Converse searched in vain.

Yet, we would argue that it is inappropriate to conclude that mass attitudes lack any sort of meaningful structure; instead, analysts must move beyond the narrow

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operationalization of constraint adopted by Converse to discover ways that citizens actually *do* organize their political attitudes.[1] Both the theoretical literature on schemata and the empirical findings of constraint research suggest that substantially more evidence of structure among political attitudes may be uncovered by adopting an approach that first, is domain specific and second, focuses on the connections between general and specific idea elements in a given domain. The recent schemata literature, for example, indicates that "domain-specific information is structured in memory in cohesive 'kernels' of thought" (Hamill, Lodge, and Blake 1985, 851). This work suggests that individuals bring different rules, criteria and processes to bear in different policy domains. Hamill, Lodge, and Blake, for instance, find that while individuals seem to structure their attitudes on economic issues according to a "rich-poor" schema, they are more likely to employ an ideological or partisan schema to structure attitudes on noneconomic issues. It is unlikely, consequently, that individuals would employ a generic criteria, or ideology, to structure attitudes in all policy domains.

The schema literature also suggests that attitude structure within a domain is more usefully studied by focusing on the relationship between general and specific attitudes (*vertical* constraint) than by focusing on the relationship between idea elements at the same level of abstraction (*horizontal* constraint). Briefly, schema theory assumes that people are "cognitive misers" who have limited ability for dealing with information and thus must use their old, generic knowledge to interpret new, specific information. From this perspective, attitude structure centers primarily on the linkages between abstract and concrete idea elements, where the former are assumed to "constrain" the latter. This emphasis is reminiscent of Converse's (1964) original conceptualization of (psychological) constraint, which assumes that specific beliefs and attitudes derive from more "superordinate values or postures" (p. 211). This perspective provides a good fit to the available evidence generated from constraint research: studies which measure vertical constraint find considerable consistency between policy positions and more general idea elements (e.g., Conover and Feldman 1984; Lane 1962; Marcus, Tabb, and Sullivan 1974; Peffley and Hurwitz 1985), while research that measures horizontal constraint, or the relationship between attitudes at the same level of abstraction (e.g., Converse 1964), typically finds little consistency.

For these reasons, we argue that it is more productive to examine attitude structure using an intensive, domain-specific approach than an extensive, interdomain approach. We have selected the foreign-policy domain for the focus of our examination for two related reasons. In the first place, foreign-policy attitudes have traditionally been assumed to be underdeveloped and disorganized, as will be discussed. Yet, more recent studies indicate that foreign-policy attitudes do exhibit structure, even if the structure does not necessarily conform to the neat liberal-conservative pattern envisioned by Converse and others.

Second, we examine the foreign-policy domain because, at present, we lack an adequate understanding of its antecedents and correlates. Standard approaches to the study of domestic attitudes are often found to be lacking when applied to foreign affairs. Partisan and liberal-conservative identifications, for example, are typically poor predictors of foreign-policy attitudes. And while social cleavages and group identifications are useful in predicting many domestic issues, social groups have not been found to differ greatly on foreign-policy issues, and the differences that do arise are often difficult to explain (Gamson and Modigliani 1969;

Hamilton 1972). One's foreign-policy attitudes do not seem to be a matter of simple self-interest, either (e.g., see Lau, Brown, and Sears 1978). Our lack of knowledge in this important area is especially puzzling in light of much recent evidence that foreign-policy attitudes play a crucial role in influencing various forms of political evaluation, such as presidential voting (Aldrich, Sullivan, and Borgida 1986; Kessel 1984) and presidential support (Hurwitz and Peffley 1987).

It is our purpose to explore the properties of foreign-policy belief systems. Specifically, using a multiple indicator (LISREL) model, we estimate levels of foreign-policy issue consistency to determine whether the electorate is really as undisciplined and thoughtless in its approach to foreign-policy reasoning as suggested and to determine what general beliefs, if any, the mass public uses to structure more specific attitudes in this very important domain.

Background

The Earliest Work

The finding that U.S. citizens are not particularly sophisticated about foreign-policy matters is longstanding, going back to Almond's (1950) presentation of his "mood theory," where he argued, "foreign policy attitudes among most Americans lack intellectual structure and factual content. Under normal circumstances the American public has tended to be indifferent to questions of foreign policy because of their remoteness from every day interests and activities. . . . Foreign policy, save in moments of grave crisis, has to labor under a handicap; it has to shout loudly to be heard even a little" (pp. 69–71). A mass of survey evidence supports Almond's thesis that U.S. citizens are often unconcerned and uninformed about foreign affairs (Erskine 1963; Free and Cantril 1968; Simon 1974). Moreover, the question of whether mass foreign-policy attitudes lack intellectual structure was answered in the affirmative with the publication of Converse's 1964 study which, as noted above, found little consistency among foreign-policy attitudes and even less consistency between foreign and domestic attitudes. Just as bothersome were Converse's (1964) findings that foreign-policy attitudes were not only unstable over the waves of the 1956–58–60 National Election Studies (NES) but were even less stable than domestic attitudes. Instability was also found to be the hallmark of foreign-policy attitudes in a more recent study by Converse and Markus (1979). Taken together, then, this evidence indicates little in the way of structure, integrity, or solidity in citizens' foreign-policy cognition.

More Recent Evidence

Scholars began to reexamine the structure of foreign-policy attitudes during the Vietnam War period. One of the first of these more recent studies, conducted by Verba et al. (1967) found Vietnam-related public opinion to be "relatively orderly. . . . The correlational analysis among scales shows patterns of consistency among the population" (p. 330). A dove-hawk continuum proved to be important for organizing the attitudes of many of the respondents. Verba et al.'s findings encouraged a large literature examining the structure of foreign-policy attitudes (e.g., Bardes and Oldendick 1978; Bennett 1974; Maggiotto and Wittkopf 1981; Modigliani 1972; Wittkopf 1981), which, almost without exception, supported the conclusion that foreign-policy attitudes are substantially more organized than Converse had determined them to be.

Skeptics of this recent literature might argue that findings of attitude structure we now have several types of evidence that Converse may have seriously underestimated attitudinal stability, in general; more specifically, we also have evidence that foreign-policy opinions are every bit as stable as their domestic-policy counterparts. Achen (1975), Erikson (1978, 1979), and others argue that response inconsistencies over time are due primarily to random measurement error in the survey items used to assess these attitudes. When over-time correlations of foreign-policy attitudes in the 1956–58–60 NES panel (foreign aid, military involvement, and isolationism in Achen's study) are corrected for measurement error, they are quite high, approaching 1.0.[2]

In short, there is mounting evidence that foreign-policy attitudes are considerably more stable and organized than early research had found. This recent literature, in addition to its disagreements with the conclusions of Converse, shares several other characteristics. In the first place, the studies do not assume that foreign-policy attitudes are structured unidimensionally along the same liberal-conservative continuum on which attitudes in other domains fall. Rather, with few exceptions, people were found to use multiple dimensions to organize their foreign-policy beliefs—dimensions specific to the domain of foreign affairs, such as militarism, isolationism, and liberal internationalism, to name but a few. Second, many of the studies focus on the relationship between general and specific attitudes (vertical, rather than horizontal, constraint). Much of this work relies on exploratory factor analysis to uncover superordinate constructs (or factors) capable of explaining the covariation between specific foreign-policy opinions. And, as argued above, it is far more realistic to expect attitude structure, if it exists, to be vertical than to be horizontal. are specious, given Converse's (1964) discover that foreign-policy attitudes are so remarkably unstable over time. In fact,

Some Problems with the Literature

There is, then, impressive evidence that the foreign-policy thinking of individuals is structured; unfortunately, though, the recent literature is plagued by a number of methodological and conceptual problems that must be addressed. In the first place, as noted, much of the work employs factor analysis to uncover evidence of attitude structure, a technique with several notable shortcomings. For one, the final solution of any exploratory factor model is heavily influenced by essentially arbitrary mathematical constraints rather than substantive or theoretical ones (Long 1984). Another drawback of the technique is that interpreting the substantive content of a factor involves a certain amount of arbitrariness on the part of the analyst (Long 1984); merely supplying a label of, say, *militarism* to a factor provides no new insight into the exact meaning of the construct.

A more general problem with the research, both factor analytic and correlational, is that it is largely descriptive, with few examples of theory-guided discussion. While there is a general consensus that foreign-policy attitudes are somehow interrelated, few authors have paid much attention to the questions of why or how they are related. And while there seems to be general agreement that belief structures are largely vertical (i.e., that specific foreign-policy attitudes are bound by more abstract, general beliefs), there is very little discussion of the utility of such an organization for the individual. Thus, while the recent work in the area has been encouraging and has demonstrated the inappropriateness of Converse's methods and conclusions, a great deal more needs to be done to determine precisely how, and why, foreign-policy attitudes are linked together.

In order to answer these questions and others, we begin with a realistic appraisal of the process by which the ordinary citizen attempts to make sense out of the complex international environment. The difficulties inherent in gaining an understanding of such a complicated universe argue for a hierarchical model of political reasoning, wherein citizens use general and abstract beliefs to constrain more specific foreign-policy attitudes and preferences.

An Information-Processing Perspective

We base our investigation of how people make sense out of international relations on an information-processing approach. Grounded in cognitive-psychology and social-cognition literatures, the information-processing perspective assumes that because humans have very severe cognitive limitations, they often behave as *cognitive misers,* coping with their shortcomings by taking shortcuts whenever possible (see Fiske and Taylor 1984 for a review of this perspective). Importantly, an individual's reliance on simplification is assumed to increase with the complexity of the environment. Exceedingly simple stimuli can be processed in a relatively veridical, faithful way, even by a cognitively limited perceiver. But a complicated and confusing world requires extensive use of heuristics to render the environment interpretable and manageable.

Among the various policy domains that comprise the political environment of the average citizen, the international sphere is exceptionally complex and ambiguous. International events that occur around the globe are remote, fluid, and extraordinarily complicated. They are also ambiguous in that information regarding the international scene is often distorted or deliberately withheld from the public. Consequently, citizens are forced to rely on the assessments of U.S. political elites and media commentators, who interpret world events but who also, quite often, disagree with one another. In short, the complexity of international affairs makes the policy domain a very ambiguous and difficult one for the public to follow, at least without having to pay substantial costs.

Owing to the complexity of the domain, *cognitive heuristics* (information short-cutting) is of great importance in explaining how a "typical citizen" processes foreign-policy information, makes decisions on foreign-policy issues, or structures foreign-policy beliefs. Yet the standard guidelines used to make sense of politics (and political issues) have not been found useful in the foreign-policy domain. It has frequently been argued, for example, that many individuals support policies that enhance their well-being or self-interest. However, the connections between one's self-interest—narrowly defined—and foreign affairs are, doubtless, even more difficult to determine than in the domestic sphere. Thus, it is not surprising that studies have found only very weak relationships between self-interest and foreign-policy attitudes (Mueller 1973; Lau, Brown, and Sears 1978). Other common criteria by which policies are often evaluated include partisanship, liberal-conservative ideology, and social class. But, as noted previously, Converse found that foreign-policy attitudes are not organized along the liberal-conservative dimension; as well, he found virtually no systematic or predictable difference between Democrats and Republicans on foreign-policy issues, a finding replicated in more recent years (e.g., Pomper 1975; Verba et al. 1967). And with the exception of isolationist sentiments, foreign-policy opinions have not been found to be connected to social status (e.g., Gamson and Modigliani 1969).

In short, political judgments in the international sphere must be made under considerable uncertainty and without many of the interpretive aids commonly used in the domestic arena. It would nevertheless be a mistake to assume that in the face of this uncertainty, most citizens will refrain from holding foreign-policy attitudes or will simply embrace "official" governmental policies as their own. Following the literature on social cognition in social psychology, we assume that people cope with uncertainty in international affairs much the same way they do in other content domains—by relying on their store of general knowledge to guide the processing of more specific information.

Among researchers who study schemata, such general-to-specific reasoning has been called "top-down" or "theory-driven" processing and is usually represented by a hierarchical structure in which abstract beliefs influence the response of the individual to more specific stimuli. It is theory driven in the sense that under conditions of uncertainty, people are assumed to behave as cognitive misers by using old, generic knowledge to interpret new, specific information. General beliefs permit economical judgments to be made under uncertainty because they provide interpretations for ambiguous stimuli, select information for storage or retrieval from memory, and "fill in" missing or ambiguous information with best guesses or default values (Taylor and Crocker 1981; Tversky and Kahneman 1981).

There are several examples of such hierarchical models of political reasoning in foreign affairs. Conover and Feldman (1984) found that their sample of university students used more general schemata to organize their more specific policy preferences in foreign affairs and other domains. And in his computer simulation of foreign-policy reasoning, Carbonell (1978) investigated a hierarchical belief structure in which higher-level goals (e.g., Communist containment) partially determine more specific policy goals (e.g., increasing military strength). Finally, in his study of elite foreign-policy decision making, Jervis (1976) argued that the general beliefs held by diplomats and other experts exerted a powerful effect on their interpretations of foreign-policy events. These studies provide preliminary evidence that encourages us to investigate such hierarchical models as are common to the mass public with a conventional survey approach.

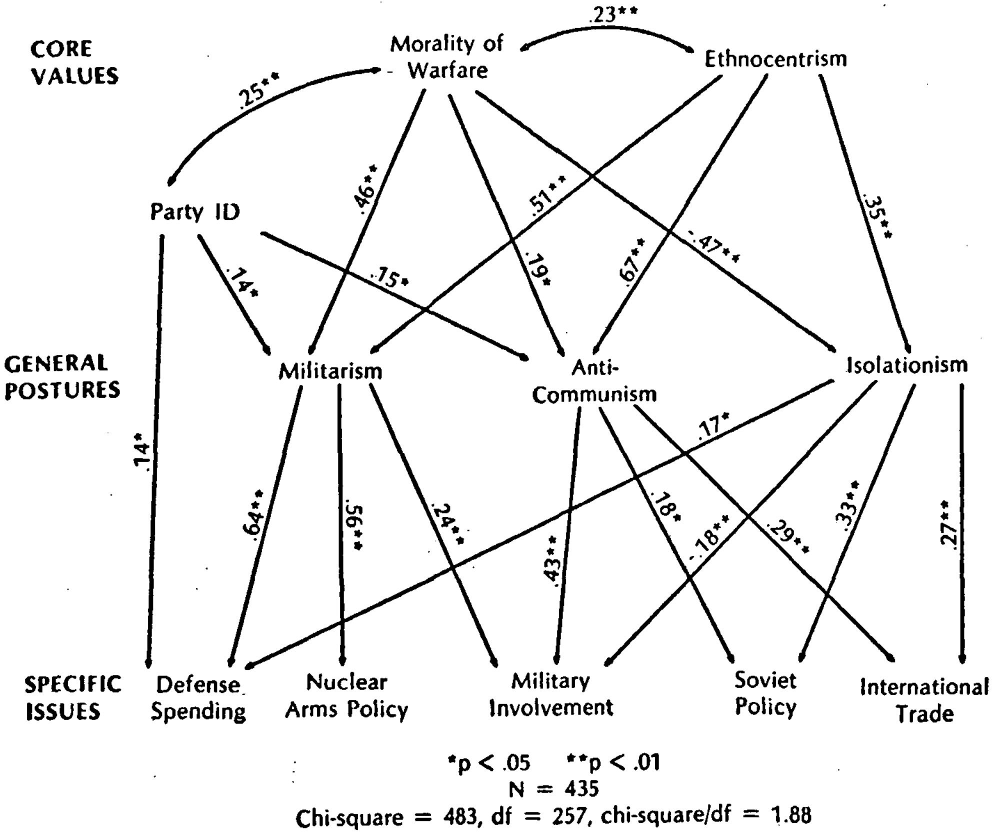
A Hierarchical Model of Foreign-Policy Attitudes

We expect citizens' foreign-policy attitudes to conform generally to the hierarchical model of constraint presented in Figure 1, where more abstract idea elements constrain more specific ones. At the most concrete level (the bottom or third tier of the figure), are preferences for specific foreign-policy issues—defense spending, nuclear-arms policy, international trade, and policies toward the USSR. Constraining these attitudes at the second (middle) tier of the attitude hierarchy are more abstract beliefs concerning the appropriate role of government in its handling of foreign affairs. These normative beliefs are collectively referred to as *postures*, for they convey the general stance or orientation the individual would like to see the government adopt in conducting foreign policy, without indicating what specific policies should be employed to attain the desired goal. Should the government adopt an aggressive or an accommodating stance in dealing with other countries? Should the government chart an internationalist or an isolationist course in international affairs? These are the basic themes defined in the factor-analytic studies. By relying on such general prescriptions to render more specific preferences, citizens are able to categorize and to evaluate economically a wide variety of concrete policies on the basis of whether those policies are consistent with their more general postures.

Thus, to use a simple example, whether an individual favors sending U.S. troops to Central America will depend in large part on whether the individual favors an isolationist or an interventionist posture in world affairs.

What more fundamental idea elements might constrain foreign-policy postures? At the first (and uppermost) tier of the hierarchy in Figure 1 are what we call *core values*, such as ethnocentrism and moral beliefs about killing in warfare. Core values are easily distinguishable from postures in that they do not refer directly to governmental actions or policies. While postures are normative beliefs regarding a government's behavior, values are more personal statements

Figure 1. Structural Relationships between Foreign-Policy Attitudes in Hierarchical Model of Constraint



CORE VALUES
GENERAL POSTURES
SPECIFIC ISSUES
Morality of Warfare
Ethnocentrism
Party ID
Militarism
Anti-Communism
Isolationism
Defense Spending
Nuclear Arms Policy
Military Involvement
Soviet Policy
International Trade
.23**
.25**
.46**
.51**
.79*
.67**
-.47**
.35**
.74*
.15*
.14*
.64**
.56**
.24**
.43**
.17*
.78*
-.18**
.29**
.33**
.27**
*p < .05 **p < .01
N = 435
Chi-square = 483, df = 257, chi-square/df = 1.88

Note: Coefficients are standardized *betas* estimated by full-information maximum likelihood. Estimates were computed with LISREL 6. All nonsignificant paths are omitted from the figure. Respondents scoring on the lower end of the variables are more *accepting of warfare, ethnocentric, Republican, militaristic, anti-Communistic, isolationist, supportive of defense spending, opposed to arms control, supportive of military involvement in Central America, opposed to relations with Russia,* and *in favor of trade restrictions.*

regarding the individual's priorities and concerns. The belief that one's country is superior to all others (i.e., ethnocentrism), for example, carries with it no explicit reference to governmental policy, in either broad or specific terms. Conceptually, values (and the fundamental beliefs that link values to more specific attitudes) have been assumed to occupy a central position in belief systems (Kinder 1983). Rokeach (1973) has argued, for example, that values "lead us to take particular positions on social issues" (p. 13), and Allport (1961) notes that "attitudes themselves depend on preexisting social values" (pp. 802–3). Several recent studies have found empirical support for this view. Values of economic individualism, racial equality, and gender equality have been found to be an important source of attitudes toward welfare policies, civil rights, and women's issues, respectively (Feldman 1983; Kinder and Sears 1981; Sears, Huddy, and Schaffer 1986; Sniderman, Brody, and Kuklinski 1984).

The particular postures and fundamental values that occupy the upper reaches of the hierarchical model will be described more fully below. At this point we wish to emphasize a characteristic shared by these two idea elements that makes them especially attractive as interpretive aids: they are "easy,"[4] largely because a deep familiarity with foreign-policy events and information is not a prerequisite to possessing a belief at either the value or the postural level. Postures should make intuitive sense to even a rank amateur in the international domain, while fundamental values, by their very nature, are based on personal beliefs that are central to one's world view and are not dependent on substantial expertise in foreign affairs.

Moreover, at least two of the dimensions, ethnocentrism and anticommunism, may rest primarily on an individual's affective orientation to the United States (i.e., ethnocentrism) and its principle adversary, the Soviet Union (i.e., anticommunism). While this affect may be based on more philosophical principles for some individuals, it need not be. By simply knowing one's likes and dislikes, it should be a relatively easy task to place oneself on dimensions like ethnocentrism and anticommunism.[5] Finally, values and postures should be relatively easy dimensions for the public to use in view of the likelihood that most public rhetoric in the area of foreign policy will be pitched at a general, symbolic level rather than at the concrete, policy level.

By way of summary, we note that investigating this hierarchical model of foreign-policy constraint allows us to overcome many of the limitations of previous research. Most importantly, in contrast to many factor-analytic studies, we directly measure the relationship between general and specific elements in a foreign-policy belief system. Our focus on foreign-policy attitudes also allows us to investigate a variety of vertical linkages between abstract and concrete elements that might be missed in a more superficial analysis. Further, we explore a common framework of ideological structure that is capable of being assessed using a conventional survey approach.

Methods

Sample and Measurement

The data for investigating the structure of foreign-policy attitudes come from a telephone survey conducted by the Survey Research Center (SRC) at the University of Kentucky, Lexington during February 1986. A variant of random-digit dialing was used to select the 501 Lexington residents who comprised the sample.[6] The professional interviewers at the SRC asked respondents a battery of questions concerning their attitudes on foreign affairs during the course of each interview, which lasted an average of 10 to 15 minutes.

To estimate our hierarchical model of foreign-policy constraint, it was necessary to measure foreign-policy attitudes at several levels of abstraction in order to tap not only specific policy preferences but also foreign-policy postures and core values. The survey questions are presented in the Appendix; the vast majority of items were pretested in a pilot study of Lexington residents ($N = 128$), most of whom were students at the University of Kentucky.

Postures

We expect an individual's concrete issue preferences to be based on three global postures that describe the general direction the government should take in international affairs: *militarism, isolationism,* and *anticommunism.* Our selection of these postures has been motivated by the work of Kegley and Wittkopf (1982), Maggiotto and Wittkopf (1981), Schneider (1984), and others who have written about foreign-policy attitudes from a historical perspective. It is the contention of these authors that the contemporary U.S. public makes two basic foreign-policy choices: (1) whether or not to become involved with other nations and (2) if involvement is desired, whether the involvement should be militaristic or nonmilitaristic (e.g., diplomatic). Consequently, we include the isolationism and militarism postures to represent these two basic choices the mass public must face.

If *isolationism* provides guidance regarding the appropriate *extent* of involvement and *militarism* provides guidance regarding the *type* of involvement, *anticommunism* provides guidance regarding the *targets* of involvement. A central question in U.S. foreign policy in the postwar period has been, What is the appropriate posture of the United States toward Communist-bloc nations? Since World War II, the Soviet Union and Communist countries have been viewed by U.S. presidents as the major international threat to the nation's interests and security (Kegley and Wittkopf 1982); and most major foreign-policy developments over this period (e.g., containment, détente, and others) have been justified as a means of coping with this threat.

All of these dimensions have consistently shown up in factor-analytic studies as superordinate constructs structuring more specific foreign-policy attitudes. Moreover, we believe these broad dimensions underlie much of the public discourse in the international sphere.[7] Our definition and operationalization of each construct deserves some elaboration.

Militarism. The dimension of *militarism* is anchored, on the one end, by a desire that the government assume an assertive, militant foreign-policy posture through military strength and on the other by a desire for a more flexible and accommodating stance through negotiations. The specific items designed to tap this dimension emphasize "toughness" as opposed to "flexibility" in dealing with other countries (A.1 in the Appendix), the view that the best way to achieve peace is through military strength rather than negotiations (A.2), and the belief that the United States should be willing to "go to the brink of war" to preserve its military dominance (A.3). We expect this dimension to be an important predictor of the contemporary divisions that exist in public support for either a defense buildup at home or the use of U.S. military force overseas.

Anticommunism. Beliefs about the appropriate posture of the United States toward Communist-bloc countries should also play a central role in shaping concrete policy attitudes (see Gamson and Modigliani 1969 for corroborating evidence on this point). Four items were used to assess *anticommunism:* respondents were asked whether the United States should do everything it can to "prevent the spread of communism" (B.1) or "check the spread of Soviet influence" (B.2) in the world, whether communism "is an acceptable form of government for some countries" (B.3), and whether or not the United States should try hard to "get along with Russia" (B.4). This posture should predict not only specific policy preferences in U.S.-Soviet relations, but also specific defense spending, military involvement, and nuclear-arms attitudes.

Isolationism. The *isolationism* posture is defined by a general desire that the government avoid any ties or entanglements with other countries, whatever the nature of the relationship (i.e., whether political, diplomatic, or militaristic). Since isolationism has longstanding roots in U.S. history (see McCormick 1985), it is not surprising that it has been used with some frequency to describe the foreign-policy orientations of the U.S. public (see, for example, McClosky 1967 and Sniderman and Citrin 1971).

While a number of isolationism scales exist, commonly used indexes either rely on questions too specific for use at the postural level (e.g., McClosky 1967) or rest on the questionable assumption that isolationism and internationalism lie at opposite poles of a single dimension (e.g., Watts and Free 1978). To avoid such problems, isolationism was measured by two Likert-scale items that expressed only isolationist (not internationalist) sentiments pitched at a fairly general level of abstraction. Thus, isolationists in our study are more likely to agree that "we shouldn't risk our happiness by getting involved with other countries" (C.1) and that the United States should turn inward and focus on problems at home rather than world affairs (C.2). This posture should constrain any policy that pertains to international involvement.

Specific Policy Preferences

To establish the range of applicability of our postural variables, we asked respondents to indicate their more specific foreign-policy preferences in several areas, including defense spending, nuclear-arms policy, military involvement (in Latin America), policy toward the Soviet Union, and international trade. The measurement of these more specific attitudes was an easier task, since there are a wealth of possible survey items to be culled from various national surveys, such as those used by the National Election Studies and the Chicago Council on Foreign Relations. As quick perusal of the appropriate section of the Appendix demonstrates, we followed the practice of measuring each policy attitude (except Soviet relations) with several indicators.

Core Values

One's placement on the postural dimensions is assumed to be guided largely by two core values: ethnocentrism and the morality of killing in warfare.

Ethnocentrism. Ethnocentrism is defined here as the belief that one's country is superior to all others; we expect such beliefs to underlie all three of the postures we have included in this study.[8] If an individual truly believes that the United States is vastly superior to other countries in the world, this belief would certainly bolster the idea that the appropriate posture of the government should be the aggressive pursuit of our national interests (i.e., militarism) as well as the protection of our system from the threat posed by Communist countries, whose interests and values are seen to be antithetical to ours. Moreover, to the degree that ethnocentrism fosters a self-centered or parochial view of the world, the tendency may be to draw inward into an isolationist shell rather than to push outward in the world.

The questions measuring ethnocentrism appear in the Appendix; they ask respondents whether other countries should try to make their governments as much like ours as possible (I.1), whether they agree that our government is the best form of government yet devised (I.2), whether other countries have governments that are just as good as ours (I.3), and how the United States compares with other countries in the world (I.4).

Morality of Warfare. Beliefs about the morality of killing in warfare are also presumed to constrain the postures described above. Clearly, one's willingness to endorse a militaristic posture should be related to one's values regarding the "rightness" of military solutions. Moreover, a strong sense that warfare is wrong should lead individuals to reject internationalism, given the tendency for many types of involvements to become military in nature. And finally, we would expect those most wary of warfare to advocate more restraint in dealing with the Soviets given the possibility that an aggressive containment policy with the Soviet Union could result in exacerbating, rather than alleviating, international tensions.

Briefly, an individual's values concerning the morality of warfare—whether taking the life of one's enemy is viewed as unconscionable or whether it is seen as a matter of necessity, perhaps even virtue—were determined by asking respondents whether it is "acceptable to kill one's enemies when fighting for one's country" (J.1) and whether a "person who loves his fellow man should refuse to fight in any war" (J.2).

As was the case with our selection of postures, we incorporated these two values into our model on theoretical grounds. It can be seen that we selected values of large range, which should be logically linked to all three of the postural positions. This is not to suggest that our list of values is exhaustive, as other values (liberty, for instance) can and should be explored.

Methodology

We used Joreskog's (1973) method for the analysis of covariance structures (LISREL) to estimate the hierarchical model of foreign-policy attitudes.[9] The covariance-structures model holds several advantages over other techniques used to estimate attitude structure and constraint. For one, the arbitrary mathematical constraints imposed on the exploratory factor model are replaced in LISREL with substantive constraints that can be "tested" with the data at hand. In addition, when one has multiple indicators of many of the idea elements in the constraint model, as we do, problems of measurement error that have plagued earlier studies of constraint (see, e.g., Achen 1975; Erikson 1978, 1979) are not so severe, since the structural relationships between latent idea elements estimated by LISREL are, in a sense, corrected for attenuation due to random measurement error.

Model Specification

Our model assumes that more concrete idea elements are constrained, or determined by more abstract attitudes at the next higher level of abstraction. Thus, each of the postures is assumed to be constrained by both of the core values, and each of the specific policy attitudes is assumed to be constrained by all three of the postures. The exogenous variables in the model, the two core values, are linked by unexplained covariances.

Partisanship and liberalism-conservatism were also included in the model as exogenous variables influencing postures and policy preferences alike. These variables, measured in the standard seven-point format developed by the NES-SRC, are included in the model for two reasons: We have noted, first, that past research suggests that foreign-policy attitudes do not seem to be organized around these more domestic orientations. We should like to see whether this independence holds for the foreign-policy attitudes measured in this study. Second, these factors serve as control variables in the model to ferret out spurious associations between foreign-policy attitudes related only through their mutual dependence on partisanship and liberalism-conservatism.[10]

Findings

The estimated epistemic correlations between the indicators and the latent idea elements are presented in Table 1. Generally, the indicators appear to be fairly reliable measures of the theoretical constructs.

Figure 1 presents the LISREL estimates of the structural relationships between idea elements at different levels of

Table 1. Epistemic Correlations between Indicators and Factors

Theoretical Construct	Indicator	Epistemic Correlations	Construct R^2
General Postures			
A. Militarism	1. be tough or be flexible	.55	.69
	2. negotiate or maintain military strength	.50	
	3. maintain dominance	.55	
B. Anticommunism	1. contain communism	.75	.61
	2. contain soviets	.65	
	3. communism acceptable	.41	
	4. get along with Russia	.25	
C. Isolationism	1. avoid involvements	.60	.30
	2. not worry about world affairs	.75	
Specific Issues			
D. Defense spending	1. level of spending	.61	.52
	2. defense or nondefense cuts	.69	
E. Military involvement	1. Central America	.78	.37
	2. El Salvador	.73	
F. Nuclear arms	1. nuclear freeze	.56	.43
	2. nuclear-weapon increase	.77	
G. Soviet relations	1. cultural ties	1.0	.18
H. International trade	1. import restriction	.78	.18
	2. job protection	.62	
Core Values			
I. Ethnocentrism	1. others should imitate U.S.	.61	
	2. U.S. government best	.46	
	3. other governments as good	.46	
	4. how U.S. compares	.36	
J. Morality of war	1. killing acceptable	.58	
	2. should refuse to fight	.55	

Note: Coefficients are standardized *betas* estimated by full-information maximum likelihood. Estimates are computed from LISREL 6.

abstraction. Overall, the hierarchical model fits the data quite well. The *chi*-square-to-degrees-of-fit statistic is 257 and, with 482.87 degrees of freedom, the *chi*-square-to-degrees-of-freedom ratio is 1.88.[11] The most general feature to note about the results in Figure 1 is the high level of interconnectedness between foreign-policy idea elements. In almost every case, attitudes at the postural and specific policy levels are significantly tied to at least two of the idea elements measured at a higher level of abstraction. And in several instances, the linkages are quite strong. Finally, as indicated by the r-squares in Table 1, a substantial proportion of the variation in most of the endogenous attitudinal elements (i.e., postures and specific policy preferences) is "explained" by more abstract beliefs (i.e., core values and postures, respectively). This is no small achievement in light of the expectations created by prior studies. Aside from the factor-analytic studies, a generation of research has cast severe doubt on the prospect of finding any evidence of consistency among foreign-policy attitudes. The general pattern of our results, however, clearly indicates that when constraint is measured as a series of vertical relationships between attitudes at different levels of abstraction, the degree of structure among foreign-policy attitudes is generally impressive.

The size and significance of the relationships between foreign-policy attitudes stand in striking contrast to the estimated linkages between these attitudes and partisanship and liberalism-conservatism. Our results thus echo those of previous studies, which failed to detect much evidence of a connection between these identifications and foreign-policy preferences; not only are most of the linkages statistically insignificant (and thus omitted from the figure) but those that are significant are uniformly low. Independent of their core values, for example, Republicans are only slightly more likely than Democrats to opt for militaristic and anti-Communist postures in the international arena; and independent of their postures, they are only marginally more likely to favor increases in defense spending. Significant liberal-conservative differences (not shown) emerge only on the *militarism* dimension.[12]

Core Values and Postures

A closer inspection of the connections among foreign-policy attitudes turns up a number of interesting findings. Focusing first on the many arrows connecting idea elements at the top of Figure 2, it is evident that individuals' core values are an important foundation of their foreign-policy postures. Collectively, the two values (along with party identification and liberalism-conservatism) account for between 30% and 70% of the variance in the postures.

The relationships uncovered here provide valuable insights into the various ways that core values anchor the general orientations people develop to guide their foreign-policy thinking. And for the most part, the linkages that emerge are consistent with our conceptualization of the particular postures and core values included in the study. For example, as one might predict, respondents desiring a more militant stance in world affairs are likely to reject the idea that killing in warfare is immoral ($\beta = .46$); they also tend to endorse the ethnocentric view that their country is without equal in the world ($\beta = .19$).

Both of the core values affect the degree to which an individual prefers an isolationist posture in world affairs. One fairly strong basis for isolationism is an expressed concern about killing in warfare ($\beta = -.47$). The implication is that the desire to retreat from international affairs may stem, in part, from apprehension about the possibility of international involvements leading to military engagements. We also find that a self-centered belief in the superiority of the United States (β = .35) underlies isolationist currents in public attitudes.

Ethnocentric values also contribute heavily to individuals' anti-Communist orientations (β = .67). On reflection, an anti-Communist posture seems a natural counterpart to an ardent faith in the "American way." As Levinson (1957) has argued, "Ethnocentric thinking in the sphere of international relations, like other forms of ethnocentrism, is based on a rigid and pervasive distinction between in-groups and out-groups . . . the focal out-group at any time being those nations who are seen, rightly or wrongly, as different from ours. . . . Other nations are seen as inferior, envious and threatening" (pp. 38–39). For many U.S. citizens, the longstanding "out-group" in foreign affairs has been the Soviet Union and other Communist countries, which many minds represent as an anti-ideal group whose interests and values run contrary to their own.

Postures and Specific Issues

A casual inspection of the lower portion of Figure 2 reveals that the three postures are crucial considerations in guiding an individual's preferences on a variety of concrete issues in foreign affairs. The effect of isolationism is as one might expect: the tendency is to reject consistently those foreign-policy options that call for more active involvement between the United States and other countries. Thus, isolationists, more so than nonisolationists, are opposed to committing U.S. troops overseas (β = −.18) and to renewed cultural relations with the Soviet Union (β = .33) but favor the imposition of trade barriers to protect U.S. jobs (β = .27). On the other hand, isolationists are in favor of increased defense spending. Though this relationship is not particularly large (β = .17), it is apparent that the isolationist's preferred method of responding to external threat is to increase the U.S. arsenal on the home front (probably for *defensive* purposes) rather than to increase military (*offensive*) commitments overseas. In sum, while the *isolationism* dimension is a more powerful determinant of nonmilitary positions than of military positions, it still appears to provide an important cue in guiding people's preferences across a wide range of different types of issues.

An individual's affinity for an assertive, versus an accommodating, posture by the government is primarily an important predictor of military-type issues. Very clear and strong differences arise on the issues of defense spending and nuclear policy between militarists and accommodationists, with the former being much more likely to favor general increases in defense spending and to oppose arms policies that require the U.S. to cut back its nuclear arsenal. Individuals who favor a more militant U.S. posture in the abstract also tend to favor sending U.S. troops to various trouble spots under more specific circumstances.

The third posture, anticommunism, shows up as a significant predictor of both military and nonmilitary issues. Individuals favoring a tougher posture toward Communist countries are more likely than not to favor sending U.S. troops to Latin America (β = .43) and placing restrictions on foreign imports (β = .29) but oppose resuming cultural ties with the Soviet Union (β = .18). The rather modest size of the coefficient for Soviet policy may be explained by the tendency for our postural variable to reflect more militant sentiments toward the containment of communism, sentiments that may not be relevant for the rather noncontroversial and diplomatic policy expressed in our measure of Soviet policy.

The absence of significant linkages between the anti-Communist posture on the one hand and defense spending and nuclear policy on the other is surprising. Nuclear-arms policies presumably involve the Soviet Union in some way and proposals to increase defense spending are often justified on the grounds that they provide a necessary deterrent to the Soviet Union and its allies. Our findings suggest, however, that one's position on these issues is, first and foremost, a reflection of the degree to which one favors an assertive, versus an accommodating, approach to foreign affairs. It may be that defense-spending and nuclear-policy issues are accounted for quite well by the *militarism* dimension, leaving little room for influence from *anticommunism*, which, after all, overlaps considerably with the *militarism* dimension ($r = .60$).[13]

The Degree of Specific-Issue Constraint

Our data also shed some light on the question of what kinds of foreign-policy issues are more apt to be grounded in general considerations and principles. Judging from the r-squares of the issue variables in Table 1, it is clear that individuals' positions on military issues, such as defense spending, nuclear policy, and military involvement, are better accounted for by the three postures than are the nonmilitary issues of international trade and Soviet policy. The differences here may be due to either or both of two reasons. First, it may be that the majority of postures included in this study (i.e., *militarism* and *anticommunism*) are more relevant guides for evaluating military, than for evaluating nonmilitary, issues. Yet it is also difficult to imagine general principles other than the postures measured here that might be used by the average citizen to anchor opinions on issues like international trade and diplomatic relations with the Soviet Union solidly. Liberal-conservative and partisan identifications, for example, provide no significant connective glue on these issues. Moreover, it should be noted that *isolationism* and *anticommunism* did prove to be important predictors of these issues; the problem is simply that these dimensions failed to account for a substantial proportion of the variance on the nonmilitary issues.

It seems more plausible to make a second argument, that military issues are more highly constrained because they are inherently more threatening to the public, are more often the object of media coverage, and are generally more salient in the mass public's mind. Because the public is more likely to think seriously about issues like military involvement and defense spending, these issues are more likely to be linked to general postures. Under normal circumstances, nonmilitary issues are, as a group, less likely to penetrate the mass public's awareness (Bennett 1974). It is therefore these nonsalient issues that come close to fitting the stereotype of foreign-policy attitudes found in much of the literature, where public thinking is portrayed as being relatively undeveloped and disorganized; though, even here, it is important to keep in mind that nonmilitary issues in this study were not found to be completely random or disorganized, only more so than military issues.

Conclusions and Implications

How do individuals make sense out of a bewildering international environment, given the irrelevance of standard political heuristics? To answer this question, we resisted the assumption of unidimensional ideology by earlier studies, as well as the exploratory procedures of factor analysts. Instead, using an intensive, domain-specific approach, we found the foreign-policy views of respondents in our study to be constrained to an impressive degree. Thus, we find core values like ethnocentrism and the morality of war to structure foreign-policy postures like militarism, anticommunism, and isolationism; which in turn are important determinants of individuals' preferences across a wide range of specific policies, such as defense spending, Soviet relations, involvement of U.S. troops overseas, international trade, and nuclear arms. This means that earlier findings that international attitudes are unrelated to a liberal-conservative dimension should not be interpreted to mean that foreign-policy attitudes lack structure. On the contrary, the general attitudes that constrain specific preferences are distinctive to the foreign-policy domain.

Critics might well argue that it is contradictory to view the electorate as structured and organized in its thinking when, at the same time, it is largely ignorant of the substance of such attitudes (we do *not* challenge the robust finding that citizens know little about foreign affairs). Sniderman and Tetlock (1986, 79) ask the question, "How is it possible for ordinary citizens to put together a consistent outlook on politics, given that they know so little about it?" They (and we) answer this question by emphasizing the functional nature of heuristics; that is, individuals organize information because such organization helps to simplify the world. Thus, a paucity of information does not *impede* structure and consistency; on the contrary, it *motivates* the development and employment of structure. Thus, we see individuals as attempting to cope with an extraordinarily confusing world (with limited resources to pay information costs) by structuring views about specific foreign policies according to their more general and abstract beliefs.

Our findings should be a source of comfort to those who appreciate the benefits of public involvement in governmental affairs. When Converse initiated the debate over constraint more than two decades ago, he justified the importance of the research by referring to the inherent benefits of mass-elite communication. When the attitudes citizens possess are not consistent nor tied together by an underlying ideology, the public will not be able to follow the themes and rhetoric of candidates and incumbents, and, moreover, the elected officials will not readily understand what general public impulses helped to carry them to power. The results of this study suggest that the public does possess general orientations that help to inform and anchor its opinions on specific foreign-policy issues.

The results of this study should also provide some relief to analysts who have been frustrated by the seeming "random" nature of political behavior in the international realm, where traditional perspectives for explaining behavior in domestic politics have not been very useful. The postures that respondents used to anchor their specific policy preferences should prove to be important analytical yardsticks for future efforts to interpret and predict mass reactions to foreign-policy events. It is both more powerful and more parsimonious for students of political behavior to examine these general orientations than to focus on the opinions to specific issues, which change over time.

Our study also provides some new insights into the question of when, and under what circumstances, the public will become interested (or disinterested) in world affairs and which issue preferences are likely to be more well developed than others. Some of the specific policies, such as defense spending, military involvement, and nuclear policy, are clearly better explained by the higher-level beliefs (postures) than are others (i.e., the nonmilitary issues). We speculate that one of the reasons why foreign policy appears to be salient during the current administration is that President Reagan has consistently spoken to precisely those military issues most likely to be constrained by the commonly shared postures. In comparison, President Carter focused primarily on less accessible and less salient foreign-policy issues—on human rights and nonmilitary international relations—which do not appear to be constrained by the postures. Thus, political elites who focus on military concerns or who frame their appeals in terms of popular themes, like militarism, anticommunism, and isolationism, are at least assured of a wider public audience. Taking some exception with Almond's (1950) earlier conclusions, we would argue that foreign policy does not necessarily have to "shout loudly to be heard only a little"; it simply has to speak clearly and in familiar terms, perhaps rattling a few sabers for emphasis.

Finally, there are several limitations on the present study that deserve special mention. In the first place, our sample overrepresents better-educated individuals, who may be expected to exhibit a greater degree of interconnectedness on political attitudes. Further, we must acknowledge the possibility that constraint is higher during an administration noted for its controversial, bold, and consistent style of international relations; there is little mistaking the president's feelings toward the Soviets, toward military buildup, or toward support for regimes friendly to the U.S. We mention these caveats not because we believe our findings are inherently time bound or lacking in external validity but rather because we recognize the need to replicate these results across different regimes and different populations.

Appendix: Lexington Survey Items

For *Some people . . . Others . . .* statements, respondents were asked whether they *agreed* or *disagreed* with the statement and whether they agreed or disagreed *strongly* or *not so strongly*, producing a five-point scale with *uncertain* and *don't know* responses at the midpoint of the scale. For straightforward statements, respondents were asked which viewpoint came closer to their own opinion and whether it was *somewhat close* or *very close* to their own view, producing a five-point scale with *depends* and *don't know* responses at the midpoint of the scale. *Don't know* responses were assigned a value equal to the midpoint of the scale for all of the five-point scales. On average, the percentage of *don't know* responses was fairly small—just above 3%, or about 16 cases—ranging from a low of 1% to an atypical high of 12%.

General Postures

A. Militarism

1. Some people feel that in dealing with other nations our government should be strong and tough. Others feel that our government should be understanding and flexible.
2. Some people feel that the best way to ensure peace is through military strength. Others feel that the best way to peace is to sit down with other nations and work out our disagreements.
3. The U.S. should maintain its dominant position as the world's most powerful nation at all costs, even going to the brink of war if necessary.

B. Anticommunism

1. The United States should do everything it can to prevent the spread of communism to any other part of the world.
2. The United States should do everything it can to check the spread of Soviet influence in the world.
3. Communism may have its problems, but it is an acceptable form of government for some countries.
4. Some people say that our government should try very hard to get along with Russia. Others say that

it would be a mistake to try very hard to get along with Russia.

C. Isolationism

1. We shouldn't risk our happiness and well-being by getting involved with other nations.
2. The United States shouldn't worry about world affairs but just concentrate on taking care of problems here at home.

Specific Issues

D. Defense Spending

1. Considering the situation today at home and abroad, do you think the total amount the United States is spending for defense and military purposes should be increased, kept at the present level, reduced, or ended altogether?
2. In your opinion, what should be the government's main approach in reducing the federal deficit? If you had to choose, would you rather see the government cut spending for defense or cut nondefense spending?

E. Military Involvement

1. Would you *strongly favor, not so strongly favor, not so strongly oppose,* or *strongly oppose* sending U.S. troops to Central America to stop the spread of communism?
2. Please tell me whether you would *strongly favor, not so strongly favor, not so strongly oppose,* or *strongly oppose* the use of U.S. troops if the Communists took over in El Salvador, Central America?

E. Nuclear Arms

1. Do you think the United States should freeze the production of nuclear weapons on its own, do so only if the Soviet Union agrees to a nuclear freeze as well, or do you think the United States should not freeze the production of nuclear weapons at all?
2. Would you *strongly favor, not so strongly favor, not so strongly oppose,* or *strongly oppose* building more nuclear weapons to increase the U.S. arsenal?

G. Soviet Relations

1. Would you *strongly favor, not so strongly favor, not so strongly oppose,* or *strongly oppose* resuming cultural and educational exchanges with the Soviet Union?

H. International Trade

1. Some people feel we should restrict the sale of foreign products like Japanese cars in the U.S. to protect American jobs. Others feel restrictions are wrong because they lead to higher prices for American products at home.
2. Would you *strongly favor, not so strongly favor, not so strongly oppose,* or *strongly oppose* restricting foreign imports to protect American jobs?

Core Values

I. Ethnocentrism

1. Other countries should try to make their governments as much like ours as possible.
2. While the American form of government may not be perfect, it is the best form of government yet devised.
3. Many other countries have governments that are just as good as ours.
4. How do you think the United States compares with other countries in the world? Do you think that it is really no better than many other countries, that it is better than many countries but not necessarily the best, or that it is absolutely the best country in the world?

J. Morality of War

1. It is certainly acceptable to kill one's enemy when fighting for one's country.
2. A person who loves his fellow man should refuse to fight in any war.

Notes

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1. It is not our intention to engage in Converse bashing. Because of his pioneering work in the field, all participants in the constraint debate are intellectually indebted to him.

2. Using data from a five-wave panel, Feldman (1985) also finds responses to domestic *and* foreign-policy issues to be almost perfectly stable. In addition, Feldman makes the point that the practice of measuring response stability with correlation coefficients (as practiced by Converse [1964]; and Converse and Markus [1979]) can seriously underestimate the true stability of foreign-policy attitudes because such coefficients are sensitive to the variance of the underlying variables. Because responses to domestic-affairs questions tend to be more polarized and more variable than responses to foreign-affairs items, the over-time correlations for domestic attitudes are much higher, even though they are actually no more stable than foreign-policy responses.

3. As an example, Bardes and Oldendick (1978) factor-analyze an initial pool of several hundred items to generate five factors—*militarism, nonmilitary involvement, world problems, détente,* and *international organizations*—which, they argue, organize the specific foreign-policy attitudes of most individuals.

4. We do not use this term in the same sense as Carmines and Stimson (1980).

5. See Brady and Sniderman's (1985) recent article on the tendency for people to use their affect toward various groups as simple guides for placing themselves and these groups on various issue dimensions, a method the authors term the *likability heuristic.*

6. The demographic characteristics of the sample come reasonably close to approximating those found in the Lexington population. According to the 1980 census, blacks comprise 13% of the Lexington population; our sample includes the same percentage of blacks. The percentage of women in Lexington is 52%; in our sample the percentage is 50%. More educated and affluent individuals are slightly overrepresented in our sample. The median income in Lexington is $15,915, while the median-income category in the sample is $20,000–$30,000 (the median is located at the lower end of this category, however). Respective *population* and *sample* percentages for education are *less than high school,* 29% and 14%; *completed high school,* 28% and 26%; *some college,* 18% and 26%; and *graduated college,* 25% and 34%.

7. We examined two additional postures that proved to be rather poor predictors of specific policy preferences. Both postures—nationalism and human rights—are attempts to tap the trade-off between national interests and moral considerations. The dimension of nationalism is anchored by the desire to promote the United States' interests overseas versus the desire to balance U.S. interests with the legitimate claims of other nations. Human rights was measured by agreement with the statement that the "U.S. should not support governments that abuse the human rights of its citizens, even if that country could help us militarily or economically." Neither posture proved useful as an explanation of individuals' preferences on specific foreign-policy issues.

8. Observers of the U.S. people as early as Tocqueville (1955, 235) have commented on our ethnocentric mentality: "The inhabitants of the United States . . . have an immediately high opinion of themselves and are not far from believing that they form a species apart from the rest of the human race."

9. LISREL combines elements of factor analysis found in psychometrics with structural-equation modeling found in path analysis and econometrics. In the factor-analysis portion of the LISREL procedure, the analyst specifies a measurement model where error-free latent variables are constructed from several imperfectly measured indicators. In the structural-equation portion of the LISREL model, the causal relations between the latent variables are specified. By analyzing the covariances between all the observed variables, LISREL uses maximum-likelihood-estimation techniques to obtain, simultaneously, estimates of the epistemic correlations between the latent variables and their indicators and estimates of the relationships between the latent variables.

10. The model assumes that attitudes at the same level of abstraction are related to one another only by their common dependence on more abstract attitudes. Since this assumption did not appear to be justified in the case of defense spending and nuclear policy (i.e., making cuts in the nuclear arsenal may imply making cuts in defense spending for many respondents), we allowed the disturbances of the equations for these two variables to be correlated by freeing an off-diagonal element in the *psi*-matrix of the LISREL model. To anticipate the estimation of the model below, the standardized correlated error term between these two attitudes is .25, indicating

that there is a significant source of (residual) covariation between defense-spending attitudes and nuclear-policy attitudes that is not accounted for by the three postures. We found little evidence of similar correlated disturbances between other attitudes in the model.

11. Wheaton, et al. (1977) consider a *chi*-square-to-degrees-of-freedom ratio of about 5.0 or less as "beginning to be reasonable." Two other goodness-of-fit statistics (not reported in Table 1)—the adjusted goodness-of-fit index (.893) and the root-mean-square residual (.074)—also show little evidence of model weakness.

12. The impact of ideological self-placement on militarism is .13, significant at the .05 level. Significant covariances are also found between liberalism-conservatism and ethnocentrism (.20), morality of warfare (.15), and party identification (.22).

13. Though anticommunism and militarism are related, we found little evidence of multicollinearity as a problem in these data, judging from the magnitude of the correlations between estimates of the parameters of the model (provided as part of the technical output available in LISREL 6).

14. A closer look at the foreign-policy attitudes typically used to illustrate the stereotypical view shows a preponderance of nonmilitary types of issues, such as diplomatic relations with China (e.g., Erikson, Luttbeg, and Tedin 1980) and foreign-aid policy (Converse and Markus 1979). Bennett (1974) has also noted that more salient foreign-policy-issue attitudes are more likely to be organized in the public's mind.

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POLITICAL CLEAVAGES AND CHANGING EXPOSURE TO TRADE

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Combining the classical theorem of Stolper and Samuelson with a model of politics derived from Becker leads to the conclusion that exogenous changes in the risks or costs of countries' external trade will stimulate domestic conflict between owners of locally scarce and locally abundant factors. A traditional three-factor model then predicts quite specific coalitions and cleavages among owners of land, labor, and capital, depending only on the given country's level of economic development and its land-labor ratio. A preliminary survey of historical periods of expanding and contracting trade, and of such specific cases as the German "marriage of iron and rye," U.S. and Latin American populism, and Asian socialism, suggests the accuracy of this hypothesis. While the importance of such other factors as cultural divisions and political inheritance cannot be denied, the role of exogenous changes in the risks and costs of trade deserves further investigation.

Why countries have the political cleavages they do and why those cleavages change are among the enduring mysteries of comparative politics. Among the many factors that have been adduced as partial explanations are preexisting cultural and religious divisions, the rapidity and timing of industrialization or of the grant of mass suffrage, the sequence of "crises" of modernization, the electoral system, and—most recently—the product cycle (see, inter alia, Binder et al. 1971; Duverger 1959; Kurth 1979a, 1979b; Lipset and Rokkan 1967; Rokkan 1970, 1981).

Without denying the importance of any of these variables, I want to suggest the relevance of a factor that has, until now, been widely neglected: externally induced changes—in countries with different factor endowments—in exposure to international trade.

To be sure, some studies of individual countries, and even a few comparative inquiries, have argued the significance of changing international trade in particular circumstances: one thinks, in particular, of Abraham 1981, Gerschenkron 1943, Gourevitch 1977 and 1986, Rosenberg 1943, Sunkel and Paz 1973. One author, Cameron (1978), has even suggested a relation, at least in recent decades, between exposure to trade and the rate of growth in state expenditure.

Arguing much more generally, I shall try to show that basic results of the theory of international trade—including, in particular, the well-known Stolper-Samuelson Theorem (Stolper and Samuelson 1941)—imply that increases or decreases in the costs and difficulty of international trade should powerfully affect domestic political cleavages and should do so differently, but predictably,

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in countries with different factor endowments. Moreover, I shall suggest that these implications conform surprisingly well with what has been observed about patterns of cleavage and about changes in those patterns in a great variety of countries during four periods of global change in exposure to trade, namely the "long" sixteenth century, the nineteenth century, the Depression of the 1930s, and the years since World War II.

Nonetheless, what I present here remains conjectural and preliminary. The evidence I shall be able to advance is suggestive rather than conclusive. It is principally the clarity of the logical case that seems to me to justify further refinement and testing.

The Stolper-Samuelson Theorem

In 1941 Wolfgang Stolper and Paul Samuelson solved conclusively the old riddle of gains and losses from protection (or, for that matter, from free trade). They showed that in any society protection benefits—and liberalization of trade harms—owners of factors in which that society is *poorly* endowed, relative to the rest of the world, as well as producers who use the scarce factors intensively.[1] Conversely, protection harms—and liberalization benefits—owners of factors the given society holds *abundantly* relative to the rest of the world, and producers who use the abundant factors intensively.[2] Thus, in a society rich in labor but poor in capital, protection would benefit capital and harm labor; and liberalization of trade would benefit labor and harm capital.

So far, the theorem is what it is usually perceived to be: merely a statement, if an important and sweeping one, about the effects of tariff policy. The picture is altered, however, when one realizes that exogenous changes can have exactly the same effects as increases or decreases in protection. A cheapening of transport costs, for example, is indistinguishable in its impact from an across-the-board decrease in every affected state's tariffs (Mundell 1957, 330); so is any change in the international regime that decreases the risks or the transaction costs of trade. The converse is of course equally true: when a nation's external transport becomes dearer, or its trade less secure, it is affected exactly as if it had imposed a higher tariff.

The point is of more than academic interest because we know, historically, that major changes in the risks and costs of international trade have occurred: notoriously, the railroads and steamships of the nineteenth century brought drastically cheaper transportation (Landes 1969, 153–54, 196, 201–2; Hobsbawm 1979, Chap. 3); so, in our own generation, did supertankers, cheap oil, and containerization (Rosecrance 1986, 142). According to the familiar argument of Kindleberger (1973) and others, international hegemony decreases both the risks and the transaction costs of international trade; and the decline of hegemonic power makes trade more expensive, perhaps—as, according to this interpretation, in the 1930s—prohibitively so. Analyzing a much earlier period, the Belgian historian Henri Pirenne (1939) attributed much of the final decline of the Roman Empire to the growing insecurity of interregional, and especially of Mediterranean, trade after 600 A.D.[3]

Global changes of these kinds, it follows, should have had global consequences. The "transportation revolutions" of the sixteenth, the nineteenth, and scarcely less of the mid-twentieth century must have benefited, in each affected country, owners and intensive employers of locally abundant factors and must have harmed owners and intensive employers of locally scarce factors. The events of the 1930s should have had exactly the opposite effect. What, however, will have been the *political* consequences of those shifts of wealth and income? To answer that question we require a rudimentary model of the political process and a somewhat more definite one of the economy.

Simple Models of the Polity and the Economy

I shall assume of domestic political processes only two things: (1) that the beneficiaries of a change will try to continue and accelerate it, while the victims of the same change will endeavor to retard or to halt it; and (2) that those who enjoy a sudden increase in (actual or potential)[4] wealth and income will thereby be enabled to expand their political influence as well (cf. Becker 1983). As regards international trade, (1) implies that the gainers from any exogenous change will seek to continue and to expand free trade, while the losers will seek protection (and, if that fails, imperialism);[5] (2) implies that those who gain, or are positioned to gain, economically from exogenous changes in international trade will increase their political power as well.

Economically, I propose to adopt with minor refinements the traditional three-factor model—land, labor, and capital—and to assume, for now, that the land-labor ratio informs us fully about any country's endowment of those two factors. No country, in other words, can be rich both in land and in labor: a high land-labor ratio implies abundance of land and scarcity of labor; a low ratio signifies the opposite. (I shall later relax this assumption.) Finally, I shall simply define an *advanced* economy as one in which capital is abundant.

This model of factor endowments inevitably oversimplifies reality and will require amendment. Its present simplicity, however, permits us in theory to place any country's economy into one of four cells (see Figure 1), according to (1) whether it is advanced or backward and (2) whether its land-labor ratio is high or low. We recognize, in other words, only economies that are (1) capital rich, land rich, and labor poor; (2) capital rich, land poor, and labor rich; (3) capital poor, land rich, and labor poor; or (4) capital poor, land poor, and labor rich.

Political Effects of Increasing Exposure to Trade

I shall now try to demonstrate that the Stolper-Samuelson Theorem, applied to our simple model, implies that increasing exposure to trade must result in *urban-rural conflict* in two kinds of economies and in *class conflict* in the two others.

Consider first the upper right-hand cell of Figure 1: the advanced (therefore capital-rich) economy endowed abundantly in labor but poorly in land. Expanding trade must benefit both capitalists and workers; it harms only landowners and the pastoral and agricultural enterprises that use land intensively. Both capitalists and workers—that is to say, almost the entire urban sector—should favor free trade; agriculture should on the whole be protectionist. Moreover, we expect the capitalists and the workers to try, very likely in concert, to expand their political influence. Depending on preexisting circumstances, they may seek concretely an extension of the franchise, a reapportionment of seats, a diminution in the powers of an upper house or of a gentry-based political elite, or a violent "bourgeois" revolution.

Urban-rural conflict should also arise in backward, labor-poor economies (the lower left-hand cell of Figure 1) when trade expands, albeit with a complete reversal of fronts. In such "frontier" societies, both capital and labor are scarce: hence both are harmed by expanding trade and will seek protection. Only land is abundant, and therefore only agri-

Figure 1. Four Main Types of Factor Endowments

	Land-Labor Ratio	
	High	Low
Advanced Economy	Abundant: Capital Land Scarce: Labor	Abundant: Capital Labor Scarce: Land
Backward Economy	Abundant: Land Scarce: Capital Labor	Abundant: Labor Scarce: Capital Land

culture will gain from free trade. Farmers and pastoralists will try to expand their influence in some movement of a "Populist" and antiurban stripe.

Conversely, in backward economies with low land-labor ratios (the lower right-hand cell of Figure 1), land and capital are scarce and labor is abundant. The model therefore predicts *class conflict:* labor will pursue free trade and expanded political power (including, in some circumstances, a workers' revolution); landowners, capitalists, and capital-intensive manufacturers will unite to support protection, imperialism, and a politics of continued exclusion. (Lest the picture of a rising in support of freer markets seem too improbable a priori, I observe at once its general conformity with Popkin's 1979 astute interpretation of the Vietnamese revolution.)

The reverse form of class conflict is expected to arise in the final case, that of an advanced but land-rich economy (the upper left-hand cell of Figure 1) under increasing exposure to trade. Because both capital and land are abundant, capitalists, capital-intensive industries, and agriculture will all benefit from, and will endorse, free trade; labor being scarce, workers and labor-intensive industries will embrace protection and (if need be) imperialism. The benefited sectors will seek to expand their political power, if not by disfranchisement then by curtailment of workers' economic prerogatives and suppression of their organizations.

These implications of the theory of international trade (summarized in Figure 2) seem clear, but do they in any way describe reality? I shall address that question more fully below, but for now it is worth observing how closely the experience of three major countries—Germany, Britain, and the United States—conforms to this analysis in the period of rapidly expanding trade in the last third of the nineteenth century; and how far it can go to explain otherwise puzzling disparities in those states' patterns of political evolution.

Germany and the United States were both still relatively backward, that is, capital-poor, societies: both, in fact, imported considerable amounts of capital in this period (Feis 1965, 24–25 and Chap. 3). Germany, however, was rich in labor and poor in land; the United States, of course, was in exactly the opposite posi-

Figure 2. Predicted Effects of Expanding Exposure to Trade

	Land-Labor Ratio	
	High	Low
Advanced Economy	Class cleavage: Land and capital free-trading, assertive Labor defensive, protectionist	Urban-rural cleavage: Capital and labor free-trading, assertive Land defensive, protectionist (Radicalism)
Backward Economy	Urban-rural cleavage: Land free-trading, assertive Labor and capital defensive, protectionist (U.S. Populism)	Class cleavage: Labor free-trading, assertive Land and capital defensive, protectionist (Socialism)

tion. Again, the demonstration is easy: the United States imported—and Germany exported (not least to the United States)—workers.[6] The theory, of course, predicts class conflict in Germany, with labor the "revolutionary" and free-trading element and with land and capital united in support of protection and imperialism. Surely this description will not ring false to any student of German socialism or of Germany's infamous "marriage of iron and rye."[7] For the United States, conversely, the theory predicts—quite accurately, I submit—urban-rural conflict, with the agrarians now assuming the "revolutionary" and free-trading role and with capital and labor uniting in a protectionist and imperialist coalition. E. E. Schattschneider (1960) or Walter Dean Burnham (1970) could hardly have described more succinctly the history of populism and of the election of 1896.[8]

Britain, on the other hand, was already an advanced economy in the later nineteenth century, one in which capital was so abundant that it was exported in vast quantities (Feis 1965, Chap. 1). That it was also rich in labor is demonstrated by its extensive exports of that factor to the United States, Canada, Australia, New Zealand, and Africa.[9] Britain therefore falls into the upper right-hand quadrant of Figure 1 and is predicted to exhibit a rural-urban cleavage, with fronts opposite to those found in the United States: capitalists and labor unite in support of free trade and in demands for expanded political power, while landowners and agriculture support protection and imperialism.

While this picture surely obscures important nuances, it illuminates a crucial difference between Britain and, for example, Germany in this period: in Britain, capitalists and labor *did* unite effectively in the Liberal party and forced an expanded suffrage and curtailment of (still principally landowning) aristocratic power; in Germany, with liberalism shattered (Sheehan 1978), the suffrage for the powerful state parliaments was actually contracted, and—far from eroding aristocratic power—the bourgeoisie grew more and more *verjunkert* in style and aspirations.

Political Effects of Declining Exposure to Trade

When declining hegemony or rising costs of transportation substantially constrict external trade, the gainers and losers are simply the reverse of those under increasing exposure to trade: owners of locally scarce factors prosper, owners of locally abundant ones suffer. The latter, however, can invoke no such simple remedy as protection or imperialism; aside from tentative "internationalist" efforts to restore orderly markets (Gourevitch 1986, Chap. 4), they must largely accept their fate. Power and policy, we expect, will shift in each case toward the owners and intensive users of scarce factors.

Let us first consider the situation of the highly *developed* (and therefore, by our earlier definition, capital-rich) economies. In an economy of this kind with a high land-labor ratio (the upper left-hand cell of Figure 1), we should expect intense *class* conflict precipitated by a newly aggressive working class. Land and capital are both abundant in such an economy; hence, under declining trade, owners of both factors (and producers who use either factor intensively) lose. Labor being the only scarce resource, workers are well positioned to reap a significant windfall from the protection that dearer or riskier trade affords; and, according to our earlier assumption, like any other benefited class they will soon try to parlay their greater economic into greater political power. Capitalists and landowners, even if they were previously at odds, will unite to oppose labor's demands.

Quite to the contrary, declining trade in an advanced economy that is labor rich and land poor (the upper right-hand cell of Figure 1) will entail *urban-rural* conflict. Capital and labor, being both abundant, are both harmed by the contraction of external trade. Agriculture, as the intense exploiter of the only scarce factor, gains significantly and quickly tries to translate its gain into greater political control.

Urban-rural conflict is also predicted for backward, land-rich countries under declining trade; but here agriculture is on the defensive. Labor and capital being both scarce, both benefit from the contraction of trade; land, as the only locally abundant factor, retreats. The urban sectors unite, in a parallel to the "radical" coalition of labor-rich developed countries under expanding trade, to demand an increased voice in the state.

Finally, in backward economies rich in labor rather than land, class conflict resumes, with labor this time on the defensive. Capital and land, as the locally scarce factors, gain from declining trade; labor, locally abundant, loses economically and is soon threatened politically.

Observe again, as a first test of the plausibility of these results—summarized in Figure 3—how they appear to account for some prominent disparities of political response to the last precipitous decline of international trade, the Depression of the 1930s. The U.S. New Deal represented a sharp turn to the left and occasioned a significant increase in organized labor's political power. In Germany, a depression of similar depth (gauged by unemployment rates and declines in industrial production [Landes 1969, 391]) brought to power first Hindenburg's and then Hitler's dictatorship. In both, landowners exercised markedly greater influence than they had under Weimar (Abraham 1981, 85–115 and Chap. 4; Gessner 1977); and indeed a credible case can be made out that the rural sector was the principal early beneficiary of the Nazi regime (see, inter alia, Gerschenkron 1943, 154–63; Gies 1968; Holt 1936, 173–74, 194ff.; Schoenbaum 1966, 156–63).[10] Yet this is exactly the broad difference that the model would lead us to anticipate if we accept that by 1930 both countries were

Figure 3. Predicted Effects of Declining Exposure to Trade

	Land-Labor Ratio	
	High	Low
Advanced Economy	Class cleavage: Labor gains power. Land and capital lose. (U.S. New Deal)	Urban-rural cleavage: Land gains power. Labor and capital lose. (Western European Fascism)
Backward Economy	Urban-rural cleavage: Labor and capital gain power. Land loses. (South American Populism)	Class cleavage: Land and capital gain power. Labor loses. (Asian & Eastern European Fascism)

economically advanced—although Germany, after reparations and cessions of industrial territory, was surely less abundant in capital than the United States—but the United States remained rich in land, which in Germany was scarce. Only an obtuse observer would claim that such factors as cultural inheritance and recent defeat in war played no role; but surely it is also important to recognize the sectoral impact of declining trade in the two societies.[11]

As regards the less-developed economies of the time, it may be profitable to contrast the Depression's impact on such South American cases as Argentina and Brazil with its effects in the leading Asian country, Japan. In Argentina and Brazil, it is usually asserted (Cardoso and Faletto 1979, 124–26 and Chap. 5; Skidmore and Smith 1984, 59–60; Sunkel and Paz 1973, 352–54), the Depression gave rise to, or at the least strengthened, "Populist" coalitions that united labor and the urban middle classes in opposition to traditional, landowning elites. In Japan, growing military influence suppressed representative institutions and nascent workers' organizations, ruling in the interest—albeit probably not under the domination—of landowners and capitalists (Kato 1974; Reischauer 1974, 186–87, 195–99). (Similar suppressions of labor occurred in China and Vietnam [Clubb 1972, 135–40; Popkin 1979, xix, 215].)

In considering these contrasting responses, should we not take into account that Argentina and Brazil were rich in land and poor in labor (recall the extent of immigration, especially into Argentina), while in Japan (and, with local exceptions, in Asia generally) labor was abundant and land was scarce (respectively, the lower left- and right-hand cells of Figure 3)?

A Preliminary Survey of the Evidence

I want now to undertake a more systematic, if still sketchy, examination of the historical evidence that bears on the hypotheses developed here. This effort will serve principally to suggest directions for further research; it can in no way be described as conclusive.

The "Long" Sixteenth Century

It has long been recognized that improvements in navigation and shipbuilding permitted, from about 1450 on, a previously unimagined expansion of trade, which eventuated in the European "discovery" and colonization of the Americas (Cipolla 1965). Among social scientists, Immanuel Wallerstein (1974) has studied this period most intensively; and it is worth emphasizing that the present analysis conforms with essential aspects, and, indeed, permits some clarification, of his.

Within the context of the age, what Wallerstein calls the *core* economies of the new world system—those, essentially, of northwestern Europe—were defined by their abundance in capital and labor, and by their relative scarcity of land. The *periphery* can be described as the exact inverse: rich in land, poor in both capital and—often leading to the adoption of slavery or serfdom—labor. Under expanding trade, the regimes of the core come to be dominated by a "bourgeois" coalition of capital and skilled labor (the Dutch Republic, the Tudors), and of the manufactures that use both intensively; the older, landed elites lose ground. Conversely, in the periphery, land—in the persons of plantation owners and *Gutsherren*—suppresses both capital and labor and, indeed, almost all urban life.

So far the equation seems apt. Can we, however, not go on to define that Wallersteinian chimera, the *semiperiphery* (Wallerstein 1974, 102-7), as comprising economies that fall into the lower right-hand cell of Figure 1, economies poor in capital and land, rich in labor? That would, I suspect, accurately describe most of the southern European economies in this period; and it would correctly predict (see again Figure 2) the intense class conflict (including the German Peasants War [Moore 1967, 463-67]) and the wholly retrograde and protectionist policies adopted by a peculiarly united class of landowners and capitalists in many of these regions.[12]

The Nineteenth Century

We can again proceed regionally, generalizing on the sketch of Britain, Germany, and the United States developed earlier for this period. For the period just before the great cheapening of transportation—roughly at the middle of the nineteenth century[13]—Britain can stand as the surrogate for the advanced and labor-rich economies of northwest Europe generally, including Belgium, the Netherlands, and northern France (Hobsbawm 1962, Chap. 9; Landes 1969, Chap. 3). For this whole region, as for Britain, the model predicts that expanding trade would engender rural-urban conflict: capitalists and workers, united in support of free trade and greater urban influence, oppose a more traditional and protectionist landed sector. It does not seem to me farfetched to see the powerful liberalism and radicalism of this whole region in the later nineteenth century (Carstairs 1980, 50, 62; Cobban 1965, 21-28, 58-67; Daalder 1966, 196-98; Lorwin 1966, 152-55)—or, for that matter, much of the conflict between secularism and clericalism—in this light.

Almost all of the rest of Europe at the dawn of this period can be compared with Germany: poor in capital and in land, rich in labor.[14] (The land-labor ratio seems as a rule to have declined as one moved from north to south within the economically backward regions of Europe [see figures for 1846 in Bowden, Karpovich, and Usher 1937, 3].) As it does for Germany, the model predicts for these other countries, particularly in southern Europe, class conflict as a consequence of increasing exposure to trade: workers (including agricultural wage laborers) press for more open markets and greater influence; capitalists and landowners unite in support of protection and more traditional rule. In its main aspect, this seems to me only a restatement of a central tendency that has long been remarked, namely that class conflict in the nineteenth century came at an earlier phase of industrialization, and more bitterly, to southern and central than to northwestern Europe (e.g., Lipset 1970, 28–30; Macridis 1978, 485–87; cf. Thomson 1962, 375–78); and it seems to me a more credible account of these regions' extremism than Duverger's (1959, 238) famous invocation of an allegedly more mercurial "Latin" temperament.

The United States, finally, represents the land-rich, but labor- and capital-poor "frontier societies" of this period generally: most of both Americas, Australia, New Zealand, even those parts of central and southern Africa that would soon be opened to commercial agriculture. Here, expanding trade benefits and strengthens landowners and farmers against protectionist capitalists and workers (although, as in the United States, the protectionist forces may still prevail); rural-urban conflict ensues, precipitated by demands from the rural sector.

Again, this does not at first glance appear wide of the mark. In many of the Latin American societies, this period cemented landed rule (Skidmore and Smith 1984, 50; Sunkel and Paz 1973, 306–21); in the United States and Canada, it was characterized by conflicts between the industrial East and the agricultural West (Easterbrook and Aitken 1958, 503–4); in almost wholly agricultural Australia, trade precipitated a cleavage between free-trading landowners and increasingly protectionist rural and urban wage labor (Gollan 1955, esp. 162–69; Greenwood 1955, 216–20).

In all of these cases, as I have emphasized before, other factors were surely at work and important aspects are neglected by the present analysis; but it is essential also not to ignore the benefits and costs of expanding trade to the various sectors.

The Depression of the 1930s

Here the fit between theory and reality seems quite strong. Not only the United States but Canada, Australia, and New Zealand were by this time advanced, land-rich economies. Labor, their only scarce factor, gained from the collapse of international trade: workers became more militant, policy shifted to the left. Most Latin American societies remained land-rich but backward; and for them this was quite generally the period of "Populist" coalitions of the two scarce factors, labor and capital. In developed northern Europe, owners and exploiters of the locally scarce factor of land grew more assertive, and generally more powerful, wherever previous developments had not caused them to disappear; capitalists and workers lost ground. Finally, throughout the backward regions of the world economy, where labor was abundant and land was scarce—not only in Asia but in southern and eastern Europe—labor lost to a renascent coalition of the locally scarce factors of land and capital: in Spain, Italy, Rumania, Hungary, and Poland, to name only the most prominent cases (Carsten 1967, Chaps. 2, 5 and pp. 194–204).

After World War II

Under U.S. hegemony, and with new economies in transportation and communication, the West since World War II has experienced one of history's more dramatic expansions of international trade (Organization for Economic Cooperation and Development [OECD] 1982, 62–63). Again, the theory would lead us to expect different regional consequences.

In the developed, labor-rich and land-poor economies—including now not only most of Europe but Japan—the model would predict an "end of ideology," at least as regards issues of class: labor and capital, both beneficiaries of expanding trade, unite to advance it and to oppose any remaining pretensions to rule by the landowning groups.[15] Conversely, in the land-rich and still underdeveloped economies of Latin America, expanding trade displaces the Depression-era "Populist" coalitions of labor and capital and brings renewed influence to the landed sectors. The areas of Asia and of southern Europe that are economically backward and abundant only in labor experience labor militancy and, in not a few cases, revolutionary workers' movements. Finally, and perhaps more as a statement about the future, the few economies rich in both capital and land—principally those of North America, Australia, and New Zealand—should, as they become seriously exposed to international trade, experience class conflict and a considerable suppression of labor. Capital and agriculture will for the most part unite in support of the free trade that benefits them; labor, as the locally scarce factor, will favor protection and imperialism.

Further Implications

To the extent that the model has gained any credibility from the foregoing brief survey, it may be useful to observe some of its other implications for disciplinary riddles and conjectures. Take first Gerschenkron's (1962) observation, and Hirschman's (1968) subsequent challenge and amendment of it, that "latecomers" to economic development tend to assign a stronger role to the state. From the present perspective, what should matter more, at least among labor-rich economies, is whether development *precedes* or *follows* significant exposure to trade. In an economy that has accumulated abundant capital before it is opened to trade, capital and labor will operate in relative harmony, and little state intervention will be required. Where trade precedes development, assertive labor faces—as it did in Imperial Germany—the united opposition of capitalists and landowners. To the extent that labor wins this struggle, it will require a strong state to administer the economy; to the extent that capital and land prevail, a state powerful enough to suppress labor is needed. Either route leads to a stronger state.

Even this generalization, however, applies only to economies where labor is abundant, and land scarce. Hence Hirschman's observation that "latecomers" in Latin America do not behave as Gerschenkron predicts should not surprise us. Where land is abundant, and labor scarce—as has generally been true of the Americas—"late" economic modernization (i.e., one that follows significant exposure to trade) radicalizes owners of *land* rather than owners of labor. In such "frontier" economies, labor and capital again find themselves in the same political camp, this time in support of protection. In the absence of class conflict, no powerful state is required.

This last point, of course, sheds some light on Sombart's old question, Why is there no socialism in the United States? If this model is right, the question is appropriately broadened to, Why is there no socialism in land-rich economies? Simply put, socialism develops most readily where labor is favored by rising exposure to trade and capital is not; labor is then progressive and capital is reactionary. But labor is never favored by rising trade where it is scarce. Powerful socialist movements, the present model suggests, are confined to backward and labor-rich economies under conditions of expanding trade (the less-developed European societies in the later nineteenth century, Asia after World War II).

A third riddle this approach may help resolve is that of the coalitional basis and aims of the North in the U.S. Civil War.[16] As Barrington Moore, Jr. posed the question in a memorable chapter of *Social Origins of Dictatorship and Democracy* (1967, Chap. 3), What was the connection between *protection* and Free Soil in the platform of the Republican party or of the North more generally, and Why did so broad a coalition support both aims?

If, as seems apparent, labor was scarce in the United States, then the nineteenth century's increasing exposure to trade should have depressed, or at least retarded the advance of, wages. By definition, slaves already received a lower wage than they would voluntarily accept (Else, why coerce them?); and increased trade could reasonably be seen as intensifying, or at least as retarding the demise of, slavery. Conversely, protection in a labor-scarce economy might so raise the general wage level (while, paradoxically, also increasing returns to scarce capital) as to make manumission feasible. Hence to link protection and abolition might seem a wholly sensible strategy. Moreover, because protection in that period would benefit workers and capitalists generally, it could attract the support of a very wide coalition. At least some of the mystery seems dissolved.

Relaxing the Reliance on Land-Labor Ratios

For the sake of logical completeness, and to fill a nagging empirical gap, let us now relax the assumption that the land-labor ratio informs us completely about the relative abundance of these two factors. We admit, in other words, that a country may be rich or poor in *both* land and labor. Four new cases arise in theory if (as I suspect) rarely in practice (see Figure 4): economies may be, as before, advanced or backward (i.e., capital rich or capital poor); but they may now be rich in both land and labor or poorly endowed in both factors.

Two cases—that of the advanced economy rich in both factors and of the backward one poor in both—are theoretically improbable[17] and politically uninteresting: if all factors were abundant relative to the rest of the world, the society would unanimously embrace free trade; if all were scarce, it would agree on protection. Let us consider, then, the remaining two possibilities.

In an advanced economy where both land and labor are scarce, expanding trade will benefit only capital. Agriculture and labor—*green* and *red*—will unite in support of protection and, if need be, imperialism; only capitalists will embrace free trade. When trade contracts in such an economy, the scarce factors of land and labor gain, and capital loses, influence; farmers and peasants are likely to seek expanded mass participation in politics and a radical curtailment of capitalist power.

In a backward economy with abundant land and labor (a possibility considered explicitly by Myint [1958, 323]), change in exposure to trade again mobilizes a coalition of red and green, but with diametrically opposed positions. Expanding trade now *benefits* farmers and workers but harms capitalists; and the labor-landowner coalition pursues a wider franchise, free trade, and disempowerment of capital. Contracting trade, however, benefits only the owners of capital and injures both workers and farmers; again intense conflict between capital and the other two factors is predicted, ending in either a capitalist dictatorship or an anti-capitalist revolution.

It is tempting, if speculative in the extreme, to see in the red-green coalitions of Scandinavia in the 1930s (Gourevitch 1986, 131–35; Hancock 1972, 30–31; Rokkan 1966, 84) the natural response to trade contraction of (by then) capital-rich but land- and labor-poor economies; and, conversely, to view modern Russian history, at least until well after World War II, as that of a backward but land- *and* labor-rich economy,[18] which, in a time of expanding trade, indeed forged an anti-capitalist coalition of peasants and work-

Figure 4. Predicted Effects on Economies That Are Rich or Poor in *both* Land and Labor

	Land and Labor both Abundant	Land and Labor both Scarce
Advanced Economy	0	Expanding trade: Capital assertive, free-trading Land and labor protectionist, defensive Declining trade: Land and labor gain power. Capital loses.
Backward Economy	Expanding trade: Land and labor free-trading, assertive Capital defensive, protectionist Declining trade: Capital gains power. Land and labor lose.	0

ers and, when trade contracted, experienced (as Stalin's enemies alleged at the time) a dictatorship of state capital over both workers and farmers.

Certainly so long as we cling to the view that land can only be abundant where labor is not, and vice-versa, we can offer no trade-based account of red-green coalitions; indeed, changing exposure to trade must drive the two factors apart, for it always helps the one and hurts the other. On the one hand, this reflects reality—coalitions of labor and agriculture have been rare, and have failed even where much seemed to speak for them (e.g., in U.S. populism and on the German left); on the other, it leaves the few actual red-green coalitions, particularly those that arose in circumstances of changing exposure to trade, as standing refutations of the model.

Possible Objections

At least three objections can plausibly be raised to the whole line of analysis that I have advanced here:

First and most fundamentally, it may be argued that the effects sketched out here will not obtain in countries that depend only slightly on trade. A Belgium, where external trade (taken as the sum of exports and imports) roughly equals GDP, can indeed be affected profoundly by changes in the risks or costs of international commerce; but a state like the United States in the 1960s, where trade amounted to scarcely a tenth of GDP, will have remained largely immune (OECD 1982, 62–63).

This view, while superficially plausible, is incorrect. The Stolper-Samuelson result obtains at any margin; and, in fact, holders of scarce factors have been quite as devastated by expanding trade in almost autarkic economies—one need think only of the weavers of capital-poor India or Silesia, exposed in the nineteenth century to the competition of Lancashire mills—as in ones previously more dependent on trade. (Cf. Thomson 1962, 163–64, on the vast dislocations that even slight exposure to trade occasioned in previously isolated areas of nineteenth-century Europe.)

Second, one can ask why the cleavages indicated here should persist. In a world of perfectly mobile factors and rational behavior, people would quickly disinvest from losing factors and enterprises (e.g., farming in Britain after 1880) and move to sectors whose auspices were more favorable. Markets should swiftly clear, and a new, if different, political equilibrium should be achieved.

To this, two answers may be given. First, in some cases trade expands or contracts so rapidly as to frustrate rational expectations. Especially in countries that experience a steady series of such exogenous shocks—Europe, for example, since 1840—divisions based on factor endowments (which ordinarily change only gradually)[19] will be repeatedly revived. Second, often enough some factors' privileged access to political influence makes the extraction of rents and subsidies seem cheaper than adaptation: Prussian *Junker,* familiarly, sought (and, rather easily, won) protection rather than adjustment. In such circumstances, adaptation may be long delayed, sometimes with ultimately disastrous consequences.

Finally, it may be objected that I have said nothing about the outcome of these conflicts. I have not done so for the simple reason that I cannot: history makes it all too plain—as in the cases of nineteenth-century Germany and the United States—that the economic losers from trade may win politically over more than the short run. What I have advanced here is a speculation about *cleavages,* not about outcomes. I have asserted only that those who gain from fluctuations in trade will be strengthened and emboldened politically; nothing guarantees that they will win. Victory or defeat depends, so far as I can see, on precisely those institutional and cultural factors that this perspective so resolutely ignores.

Conclusion

I have not claimed that changes in countries' exposure to trade explain all, or even most, of their varying patterns of political cleavage. It would be foolish to ignore the importance of ancient cultural and religious loyalties, of wars and migrations, or of such historical memories as the French Revolution and the *Kulturkamf.* Neither have I offered anything like a convincing empirical demonstration of the modest hypotheses I have advanced; at most, the empirical regularities that I have noted or have taken over from such authorities as Gerschenkron and Lipset can serve to suggest the plausibility of the model and the value of further refinement and testing of it.

I have presented a theoretical puzzle, a kind of social-scientific "thought-experiment" in Hempel's (1965) original sense: a teasing out of unexpected and sometimes counterintuitive implications of theories already widely accepted (Chap. 7). For the Stolper-Samuelson Theorem *is* generally, indeed almost universally, embraced; yet, coupled with a stark and unexceptionable model of the political realm, it plainly implies that changes in exposure to trade must profoundly affect nations' internal political cleavages. Do they do so? If they do not, what is wrong —either with our theories of international trade or with our understanding of politics?

Notes

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1. In fact, the effect flows backward from products and is an extension of the Heckscher-Ohlin theorem: under free trade, countries export products whose manufacture uses locally abundant, and import products whose manufacture uses locally scarce, factors intensively (cf. Leamer 1984, esp. 8–10).

2. Admittedly, this result depends on simplifying assumptions that are never achieved in the real world, among them perfect mobility of factors within national boundaries, a world of only two factors and two goods, and incomplete specialization. Still, as an approximation to reality, it remains highly serviceable (cf. Ethier 1984, esp. 163–64, 181).

3. Later historians have, of course, largely rejected Pirenne's attribution of this insecurity to the rise of Islam and its alleged blockade of Mediterranean commerce (Havighurst 1958). It can hardly be doubted, however, that the decline of Roman power by itself rendered interregional trade far less secure.

4. As transportation costs fall, states may offset the effect by adopting protection. Owners of abundant factors then still have substantial *potential* gains from trade, which they may mortgage to pressure policy toward lower levels of protection.

5. Countries that lack essential resources can only beggar themselves by protection. Ultimately, those in such a society who seek protection from trade must advocate conquest of the missing resources—as indeed occurred in Japan and Germany in the 1930s.

6. Between 1871 and 1890, just under two million Germans emigrated to points outside Europe; in the same years, some seven million immigrants entered the United States (Mitchell 1978, Tbl. A-5; Williams, Current, and Freidel 1969, 158).

7. The Stolper-Samuelson analysis also helps to clear up what had seemed even to the perspicacious Gerschenkron (1943, 26–27) an insoluble riddle: why the *smallholding* German peasants had quickly become as protectionist as the *Junker*. Not only landowners, we now see, but all enterprises that *used land intensively*, will have been harmed by free trade. On the other hand—and later the distinction will become crucial—agricultural *wage labor* should have been free trading.

8. That the farmers of the Great Plains were hardly prospering in these years is no refutation of the analysis advanced here. Their *potential* gains were great (see n. 4), and their suffering could plausibly be attributed not to expanded trade but to the obstacles or exploitation laid upon that trade by other sectors. As in Marxist analysis, the older relations of production and of politics could be seen as "fetters."

9. Emigrants from the United Kingdom to areas outside Europe totalled 5.1 million between 1871 and 1890 (see Mitchell 1978, Tbl. A-5).

10. Certainly they had been among its earliest and strongest supporters: virtually every study of late Weimar voting patterns (e.g., Brown 1982; Childers 1983; Lipset 1960, 138–48) has found a large rural-urban difference (controlling for such other variables as religion and class) in support for National Socialism.

11. Historians have, of course, often recognized declining trade's sectoral effects on Weimar's final convulsions; the controversial essay of Abraham (1981) is only the best-known example. They may, however, have exaggerated agriculture's woes (see Holt 1936; Rogowski 1982).

12. Sabean (1969, Chap. 3) and Blickle (1981, 76–78) link the Peasants War convincingly to the density and rapid growth of population in the affected areas, i.e., to an increasing abundance of labor.

13. "The world's trade between 1800 and 1840 had not quite doubled. Between 1850 and 1870 it increased by 260 percent" (Hobsbawm 1979, 33).

14. Finer distinctions would require a more precise definition of factor abundance and scarcity. The one commonly accepted for the case of more than two factors stems from Vanek's extension of the Heckscher-Ohlin Theorem (Leamer 1984, 15); it defines a country as abundant (or scarce) in a factor to the extent that its share of world endowment in that factor exceeds (or falls short of) its share of world consumption of all goods and services. Leamer's (1984, App. D) Factor Abundance Profiles are a tentative effort to apply this definition to present-day economies. To do so with any precision for earlier periods hardly seems possible.

15. Zysman seems to me to have captured the essence of European and Japanese agricultural policy in this period: "The peasantry could be held in place [by subsidies and price supports] even as its economic and social positions were destroyed" (1983, 24).

16. I am grateful to David D'Lugo and Pradeep Chhibber for having raised this issue in seminar discussion.

17. More precisely, they are inconsistent with balanced trade (cf. Leamer 1984, 8–10; Leamer 1987, 14–15).

18. There can be no doubt of Russia's abundance of land: as late as 1960, its population per square kilometer of agricultural land (35.7) was comparable to that of the United States (40.9) or Canada (28.4) and strikingly lower than those of even the more thinly populated nations of western and central Europe (e.g., France, 133; Poland, 146) (World Bank 1983). On the other hand, Myint's (1958, 323–31)

insightful analysis suggests how even sparsely populated regions can have great reserves of underemployed labor under conditions of primitive markets and social structures; and he takes episodes of extremely rapid economic growth, such as the USSR exhibited in the 1930s, as putative evidence of such "surplus" labor (Myint 1958, 323-24, 327).

19. The chief exception to this rule arises from extensions of trade to wholly new areas with quite different factor endowments. In 1860, for example, Prussia was abundant in land relative to its trading partners; as soon as the North American plains and the Argentine *pampas* were opened, it ceased to be so. I am grateful to my colleague Arthur Stein for having pointed this out.

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PUBLIC OPINION AND THE U.S. SUPREME COURT: FDR'S COURT-PACKING PLAN

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I show the intimate connection between the actions of the justices and support for the Supreme Court during one of the most critical periods of U.S. political history, the four months of 1937 during which Franklin D. Roosevelt sought legislation to "pack" the high bench with friendly personnel. Over the period from 3 February through 10 June 1937, the Gallup Poll queried national samples on 18 separate occasions about FDR's plan. These observations constitute the core of my analyses. I demonstrate the crucial influence of judicial behavior and the mass media in shaping public opinion toward the Supreme Court. This research illuminates the dynamics of public support for the justices, contributes to a clearer understanding of an important historical episode, shows the considerable impact of the mass media on public attitudes toward the Court, and adds more evidence on the role of political events in the making of public opinion.

Political institutions require some minimal level of public support to survive and to thrive. It is, of course, exceedingly difficult to document the consequences of popular support or the lack thereof; but most political scientists seem to agree that it matters. For the Supreme Court, which has no budgetary power or military authority, standing with the public takes on an especially large dimension. Students of the Court and public opinion have emphasized the lack of information, instability of attitudes, and shallowness of support for specific decisions among the U.S. citizenry (Adamany and Grossman 1983; Casey 1974, 1976; Daniels 1973; Dennis 1975; Dolbeare 1967; Dolbeare and Hammond 1968; Kessel 1966; Murphy and Tanenhaus 1968a, 1968b; Tanenhaus and Murphy 1981). That characterization of the mass public—as ill informed about and indifferent toward the Court—undoubtedly captures the essence of opinion at the level of individuals. But in a recent article, I argued that "the responsiveness of public support for the Court in the aggregate to political events and shifts in the behavior of the justices stands in stark contrast to the conventional image of the American citizenry as out of touch with and unmoved by the Supreme Court" (Caldeira 1986, 1210).

Here I intend to show the intimate connection between the actions of the justices and support for the Supreme Court during one of the critical periods of U.S. political history, the four months of 1937 during which Franklin D. Roosevelt sought legislation to permit him to "pack" the high bench with friendly personnel. The argument is disarmingly simple: the justices themselves helped to shape events and build up institutional support with a series of well-timed decisions. More specifically, I demonstrate that in two crucial actions, the Court let the air out of FDR's sails; the president, despite a

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magical touch, had little success in persuading the public of his program's urgency; and the exposure of the proposal in the mass media expanded the "scope of conflict" and helped the justices appreciably. I do not, of course, wish to suggest that the Supreme Court routed President Roosevelt in the battle over constitutional politics in the 1930s. For, in the words of one scholar, the Court won the battle, and FDR won the war (Burns 1956). Nevertheless, the Supreme Court did influence public opinion in a significant fashion in the short run. This research is of consequence because it illustrates the dynamics of public support for the justices, contributes to a clearer understanding of an important historical episode, shows the considerable impact of the mass media on public attitudes toward the Court, and adds more evidence on the role of political events in the making of public opinion (see Dalton and Duval 1986; Kernell 1976, 1986; Lehne and Reynolds 1978; Ostrom and Simon 1985). The results suggest, again, that public support in the aggregate for the Supreme Court responds in a rational and explicable manner to the actions and decisions of political elites (see Caldeira 1986).

Quite unlike most issues, FDR's proposal forced the public to choose between the widely approved policies of an extremely popular president and the institutional integrity of a controversial Supreme Court. Seldom in U.S. constitutional history have the president and the Court gone head-to-head in battle so directly for so long (see Murphy 1962; Nagel 1965; Schmidhauser and Berg 1972; Scigliano 1971). For five months, the mass media, Congress, and the president focused on little else. Indeed the political controversy was sufficiently warm to motivate George Gallup to mount an extended series of polls of the public on the question of the proposed change. Over the period from 3 February through 10 June 1937, the Gallup Poll queried national samples on 18 separate occasions about "Court packing" and related issues. This set of observations constitutes the core of the statistical analyses in the present paper. These data have with certain exceptions lain fallow for nearly 50 years (cf. Cantwell 1946; Handberg 1984). It is, altogether, an exceedingly rich lode of data on perceptions of the Supreme Court and on the dynamics of public support for political institutions.

The Context of "Court Packing"

Scholars have told the tale, on several occasions and in considerable detail, of the fight in 1937 over President Roosevelt's proposal to pack the Court; here I review the basic chain of events before moving on to the description and analysis of the data (see Adamany 1973; Alsop and Catledge 1938; Baker 1967; Irons 1982; Leuchtenberg 1966, 1969, 1985; Mason 1956).[1] From the outset of the Roosevelt administration, the president and the Court were at cross-purposes. He did not make the decision to attack the Court overnight. The justices, or a majority of them, wanted to constrain national power; Roosevelt, with a "New Deal," set out to expand the horizons of government. After a period of peace between the two branches, the Court in May of 1935 struck down a large segment of the New Deal—the Railway Pension Act, the National Industrial Recovery Act, and the Frazier-Lemke Act—and reversed FDR's dismissal of a member of the Federal Trade Commission. Then, in January 1936, the Court invalidated part of the Agricultural Adjustment Act and in May found the Guffey Coal Act and the Municipal Bankruptcy Act contrary to the Constitution. To add insult to injury, the Court in *Morehead v. ex rel. Tipaldo* struck down the State of New York's attempt to set a minimum wage for women. These and other cases signaled a stubborn resistance on the part of the Court to the heart of the New Deal and, for that matter, the emerging modern state. In the view of four, and probably five, of the justices, neither the national nor the state governments had the constitutional capacity to take the actions FDR and others saw as necessary for economic recovery.

In May 1935, Roosevelt criticized the Supreme Court for "relegat[ing] us to the horse-and-buggy definition of inter-state commerce." Still, he took no action against the justices. Others, allies of the New Deal in Congress, introduced a variety of bills to strike at the Court or to amend the Constitution to clarify national legislative powers. Proposed methods of curbing the Supreme Court included the creation of new seats, mandatory retirement at a certain age, abolition of judicial review, the agreement of an extraordinary majority on the Court to invalidate a federal law, and a requirement of advisory opinions on the constitutionality of proposed statutes. During the latter part of 1935 and first half of 1936, the antagonism between the Court and the New Deal was a hot topic in the halls of Congress and in the press. The issue was weighty enough for the Gallup Poll to have asked the public several times in this period about restrictions on the Supreme Court's power to invalidate legislation (see Cantril and Strunk 1951; Murphy 1962, 61). Nevertheless, FDR bided his time, discussing the matter with advisors and maintaining a studied silence on the issue during the presidential campaign of 1936.

Finally, after the election, FDR and Attorney General Homer Cummings fastened upon a proposal from Professor Edward S. Corwin of Princeton; the president would request legislation to permit him to nominate one additional justice for every sitting member who had served 10 or more years and had declined to retire at the age of 70. This proposal, if enacted, would have given Roosevelt six nominations to the Court. Emerging from a veil of secrecy, the president announced the plan on 5 February to members of the cabinet, leaders of Congress, and a press conference. He added several bonuses to the bill as a smokescreen for the real purpose: additional lower federal judges, reassignment of judges by the Chief Justice to the busiest courts, and a proctor to supervise the flow of litigation. All of this FDR portrayed as a plan to make the justices more efficient and to bring them abreast of their work.

In the wake of the announcement, debate about the proposed restructuring of the Supreme Court poured forth and dominated the public agenda. Story after story—discussions of the plan from every conceivable angle—filled the nation's newspapers. Meanwhile, leaders in Congress, FDR's allies, had little enthusiasm for the program. Over the next few months, as we shall see, a number of political events shaped the ebb and flow of public support for curbing the Court.

Political Events and Public Opinion

Many ingredients influence the outcomes of political campaigns; President Roosevelt's failed attempt to "pack" the Court was the result of a complicated train of events and nonevents. And the first half of 1937 was filled with events of far-reaching historical importance (for a list, see Cantwell 1946, 925). Yet, after a careful review of the period, I have identified four crucial events as particularly instrumental in the battle between the Supreme Court and the president. These events appear over and over again in narratives on the critical junctures in the debate on the Court. Furthermore, of the train of events from February through June, this set of four was the most visible and logically related to shifts in public opinion. First, on 4 March in a speech at the Democratic Victory Dinner and again in a fireside chat on 8 March, President Roosevelt took the case for Court packing to the people. At the dinner with the faithful, he called for partisan loyalty on the issue. Over the air waves, he called attention to the Supreme Court's alleged usurpation of political power. These speeches and the attendant follow-up by lieutenants, I believe, increased public support for the plan; FDR certainly intended them to do so.

Second, on 29 March, the Court announced the decision in *West Coast Hotel v. Parrish*, upholding the State of Washington's minimum-wage law and overruling the odious precedent of *Adkins v. Children's Hospital.* This decision marked an apparent retreat from the nullification of state legislative power in *Morehead v. Tipaldo*, an opinion delivered less than a year before. The hospitable attitude the Court exhibited in *Parrish* provided a stark contrast to FDR's claims that the justices opposed all forms of social progress. Opponents of the plan, naturally, cited the decision in *Parrish* as proof in part that the nation did not need to restructure the Supreme Court.

Third, on 4 April, the Court validated a major element of the New Deal, the Wagner Act, in four separate cases; *NLRB v. Jones and Laughlin Steel Company* has come down in history as the landmark. Because of the Wagner Act's sweeping nature and the justices' previously demonstrated hostility to broad exertions of national power, the Supreme Court's decision came as somewhat of a surprise. Chief Justice Hughes articulated a vision of national power ample enough to accommodate much of the New Deal. With this display of liberality toward the regulation of interstate commerce, the Supreme Court by deed called the urgency of FDR's scheme into serious question.

Fourth, on 18 May, Justice Van Devanter announced his retirement from the Court. As one of the "Four Horsemen," Van Devanter had contributed a consistent vote in a 5–4 coalition against President Roosevelt's measures and variants of the "little New Deals" from the states. Moreover, although not a productive writer, Van Devanter functioned as the intellectual leader of the Supreme Court's conservatives. With Justice Van Devanter out of the picture and a Rooseveltian in his place, the administration could hope for a 5–4 majority in most of the crucial cases. FDR could not, of course, count on ironclad support from the justices; but the future looked much brighter after 18 May.

In sum, then, I anticipate that President Roosevelt's speeches, the Court's decisions in *West Coast Hotel v. Parrish* and *NLRB v. Jones and Laughlin Steel*, and Justice Van Devanter's departure influenced public support for the proposed restructuring. Later, I shall set out more specific expectations.

Effects of Mass Communications

Even a casual perusal of the literature quickly establishes that scholars have not arrived at a consensus on the impact of mass media in politics. There are, in fact, at least two main questions at issue: Do the media influence what people think *about*? and Do the media persuade the public in one direction or another? Until quite recently, most scholars agreed that "because individuals were firmly anchored in an original set of beliefs and social network of friends, family, and fellow workers, . . . messages from the media worked primarily to reinforce the dispositions first held" (MacKuen 1931, 21; see, for example, Klapper 1960). The press, Cohen said in a conventional assertion, "may not be successful much of the time in telling people what to think, but it is stunningly successful in telling readers what to think *about*. . . . It [puts] a claim on their attention, powerfully determining what they will be thinking about, and talking about, until the next wave laps their shore" (Cohen 1963, 13). Lamentably, the earliest researchers targeted the capacity of the mass media to influence *evaluations* of political candidates and issues to the exclusion of the *agenda-setting* function of the various modes of communication.

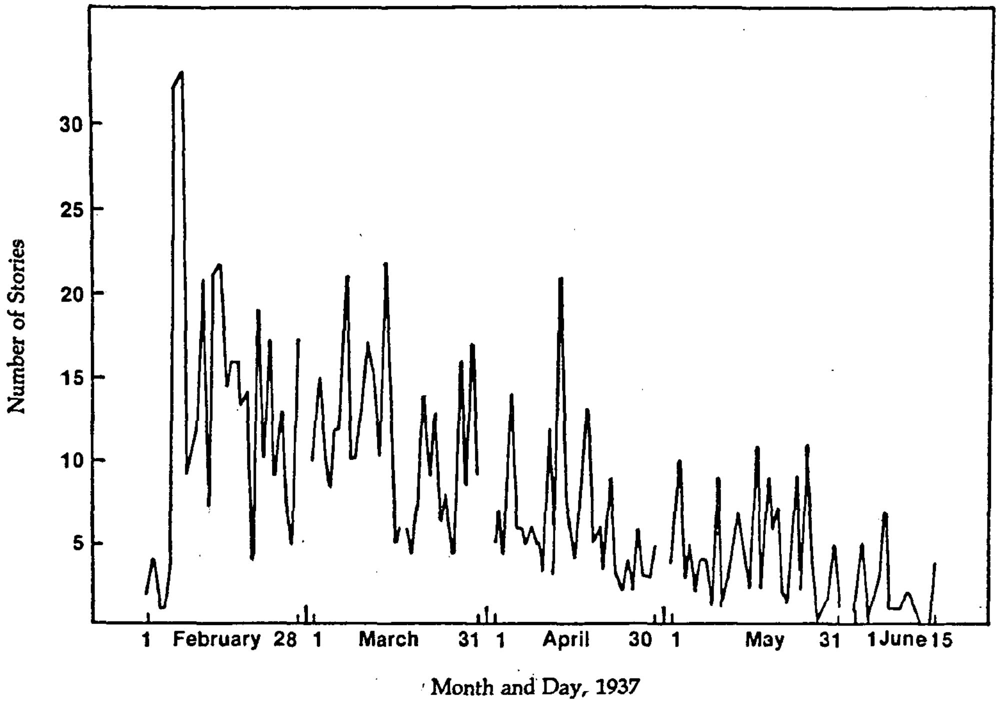
It is now clear, after a spate of scholarly activity in the 1970s, that the press and broadcasting play large roles in bringing issues and personalities to the forefront of public consciousness and keeping them there; the mass media, in sum, help to set the national agenda (e.g., Funkhouser 1973). At the crudest level, some analysts model public opinion as a simple, "mirror image" of an issue's or personality's salience in the mass media: the greater the coverage, the greater the awareness among the citizenry. Others have taken a more sophisticated approach. Thus, for example, MacKuen (1981) has demonstrated the significant impact of media attention on the concerns of the public, but he also finds events and objective conditions to be crucial elements of the equation (see also Erbring, Goldenberg, and Miller 1980; Iyengar, Peters, and Kinder 1982).

Quite apart from agenda setting, in the last few years, several students of persuasion have mounted a concerted challenge to the conventional wisdom that the mass media exercise "minimal effects" on political decisions and outcomes. In a study of newspapers during the congressional election of 1974, Miller and his associates discovered that readers of highly critical papers displayed more distrust in government than did others: "media style in reporting . . . events . . . had an effect on the degree of popular disaffection found in America" (Miller, Goldberg, and Erbring 1979, 80). Similarly, Coombs (1981) reports that in 1972 and 1974, newspaper endorsements had a measurable impact on a candidate's chances of victory at the polls.

Here I anticipate a significant relationship between the media's coverage of the Supreme Court and support for Court packing (on the Court and the press, see Grey 1972; Newland 1964). There is, unfortunately, no readily available, comprehensive indicator of the extent of the mass media's attentiveness to the Court and President Roosevelt's plan. Of the major newspapers, only the *New York Times* has an index that reaches back into the 1930s. So far as I can tell, the radio networks have not published or archived indexes of their broadcasts (but see Murrow 1937). For my indicator of the media's coverage, I have adopted the number of stories in the *New York Times.* That measure, taken weekly in the first six months of 1937, correlates strongly with the number of stories on the Court collected in *The Reader's Guide to Periodical Literature.* To be sure, the number of stories in the *New York Times* at best taps indirectly the amount and kind of information the public possesses on a particular subject; after all, its readers constitute a highly educated, elite stratum of U.S. society. Nevertheless, as the correlation between coverage in the *New York Times* and *The Reader's Guide* attests, the coverage in this elite newspaper tracks closely with other sources of information. Furthermore, reports in the *New York Times* undoubtedly diffuse throughout the nation in a "two-step flow" of information. (For a defense of a similar measure, see MacKuen 1981, 63-64.)

Figure 1 displays the number of stories on the Supreme Court in the *New York Times* on a daily basis from 1 February through 15 June 1937. Coverage of the Supreme Court varied a good deal in intensity, from a high in early February to a low in May, and declined across the period. Discussion of the Court did occur daily until late in May, so the public was subjected to a continuous barrage of

Figure 1. Coverage of Supreme Court, 1–15 February



information. The peaks in coverage, naturally, correspond to key events in the campaign over Court packing. Thus, for example, the announcement of the decision in *NLRB v. Jones and Laughlin Steel* set off a raft of stories on it and related matters. What is most remarkable here is the length and intensity of attention to the Supreme Court. Surely the Court has not since then surfaced so long and so prominently on the public agenda, even during the salad days of the Warren Court.

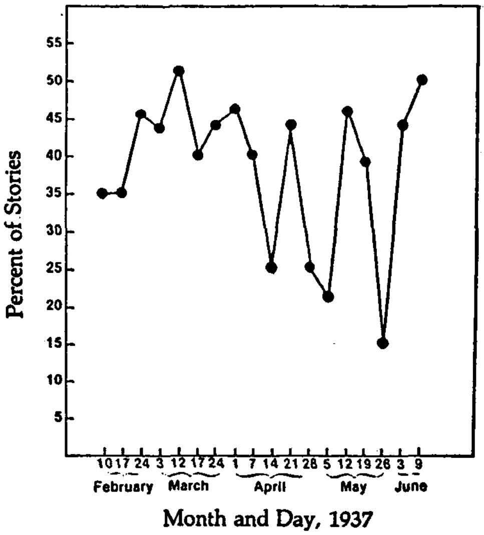
Specifically, I expect a negative relationship between the intensity of coverage and support for Court packing. Public attention to the Court operated, I believe, in classic fashion; Schattschneider wrote of the "socialization of conflict" (1960; see also Truman 1951). That is, as the mass media focused on the issue, more and more groups, institutions, and individuals entered into the fray. After a reading of the news for that period, one gathers the cumulative impression, especially in the latter half, of a cacaphony of voices and actors for and against the plan. Enemies of FDR and of Court packing did all they could to mobilize potential opponents. And, of course, as more interest attended the issue, more questions inevitably arose. In his study of presidential popularity, Mueller (1973) has characterized a similar phenomenon over the course of a chief executive's term as a "coalition of minorities"; the more exposure a president receives, the more critics he acquires. In any event, the number of stories in the *New York Times* provides a neat index of the expansion of conflict. Extended focus on a subject should, in addition, foster more coherence among the citizenry. Accordingly, media coverage did indeed help to crystallize public opinion toward the president's plan; as the number of stories increased, the percentage of the public with no opinion decreased ($b = -0.06$; $r^2 = .15$).

Figure 2 portrays the directional balance of stories on the Supreme Court in the *New York Times* from February through mid-June 1937. To create this variable, I have divided stories into categories of *positive*, *negative*, and *neutral* toward the Supreme Court. I then excluded the neutral reports. The percentages in Figure 2 represent the portion of the remaining stories I could classify as supportive of Court packing.[2] Throughout the period under consideration, the coverage of the issue ran against President Roosevelt. In only two weeks did favorable views of Roosevelt's position on the Court hit 50% or higher—immediately after his fireside chat and in the final part of the series as interest in the issue faded. Actually, these last two observations are probably somewhat misleading because the number of stories by that time had decreased to a handful. On average, about 39% of the categorized stories placed FDR's proposal in a favorable light. If these data are at all representative of the mass media's treatment of Court packing—and I believe they are—then FDR clearly received little aid and comfort from the press.[3] Naturally, I expect that as the content of coverage became more positive toward the proposal, the segment in the nation in favor of Court packing grew apace. There is, incidentally, no relationship between the level of media coverage and the direction of the stories.

Methods, Models, and Data

To test for the effects of the four events, I have chosen a multiple interrupted time-series design (see McDowall et al. 1980).

Figure 2. Direction of Stories on Court Packing



In its most elementary form, interrupted time-series analysis "attempts to answer a simple question: Does the occurrence of a particular event change a variable's behavior over time?" (Lewis-Beck 1986, 209). Since I specify other independent variables, the model differs from the typical one. Under the best circumstances, I would include for each event a term to pick up a shift in the slope and one to detect a change in the intercept.[4] Two considerations militate against that strategy here. First of all, the number of observations involved ($N = 18$) is small, and the period of time short (18 weeks). The inclusion of nine parameters for the four events (two for each plus one for the trend), as normal practice indicates, brings on unacceptable levels of multicollinearity and exhausts degrees of freedom. Signs of coefficients go astray. Second, even if full specification were a technical option, I would probably argue for a more limited and parsimonious approach. Because of the briefness of the series in question, a shift in the slope for an event seems implausible. Instead, we need to monitor sudden, step-level changes in public opinion as a result of the four political events—in other words, to watch for shifts in the intercept.

These ruminations imply the following equation to model the hypothesized consequences of political occurrences:

$$S_t = b_0 + b_1 FDR_{1t} - b_2 PAR_{2t} - b_3 NLRB_{3t} - b_4 VDV_{4t} - b_5 TREND_{5t} + e_t$$

where S_t = 18 time-series observations on the dependent variable, percentage in the sample in favor of Court packing; FDR_{1t} = a dichotomous dummy variable scored 0 for observations before 10 February and 1 after the fireside chat; PAR_{2t} = a dichotomous variable scored 0 before 1 April (the Court announced *West Coast Hotel v. Parrish* on 29 March) and 1 thereafter; $NLRB_{3t}$ = a dummy variable denominated 0 before 12 April and 1 afterward (*NLRB v. Jones and Laughlin Steel* came down on 12 April); VDV_{4t} = a dichotomous variable scored 0 before Justice Van Devanter's resignation on 18 May and 1 thenceforth; and $TREND_{5t}$ = a dummy variable counter for time from 1 to 18. In this model, b_1, b_2, b_3, and b_4 estimate any post intervention changes in the intercept. So, for instance, b_1 represents the immediate impact of President Roosevelt's speech. Based on the considerations outlined in the Political Events and Public Opinion section, I anticipate $b_1 > 0$ and b_2, b_3, and $b_4 < 0$.

In the previous section, I discussed the sources and properties of the two indicators of the mass media's treatment of the Supreme Court. For the direction of stories in the *New York Times*, as in Figure 2, I rely on a simple percentage. The indicator of media attention poses one problem. With such a profusion of stories at the outset of the period (see Figure 1), the series contains a formidable trend downward ($NYST_t = 83.9 - 4.24\ TREND_t$; $r^2 = .85$). The series is, in econometric parlance, "nonstationary." To make the series stationary, I have differenced the number of stories—change in attention is a function of this week's coverage minus last week's coverage. The final model, then, is

$$S_t = b_0 + b_1 FDR_{1t} - b_2 PAR_{2t} - b_3 NLRB_{3t} - b_4 VDV_{4t} - b_5 TREND_{5t} + b_6 NYBAL_{6t} - b_7 CNYST_{7t} + e_t$$

where the notation through $TREND_{5t}$ remains as before; $NYBAL_{6t}$ = the balance of stories in the *New York Times* in favor of Court packing and FDR's approach to the Court, counted weekly, in percentages; $CNYST_{7t}$ = the change in the number of stories from one period to the next; and e_t is a term for errors. On the basis of the discussion in the Effects of Mass Communications section, I expect $b_6 > 0$ and $b_7 < 0$. In the initial runs, the coefficient for $TREND_{5t}$ failed to reach statistical significance; therefore, in the interest of conserving degrees of freedom, I dropped it in subsequent analyses. To estimate the coefficients, I have used a method equivalent to generalized least squares.[5]

Now is the time to cast an eye on and describe the dependent variable, support for Court packing. Figure 3 records support for, opposition to, and lack of, opinion on FDR's plan among 18 national samples.[6] Support for FDR's proposal varied from a high of 46% immediately after his fireside chat to a low of 31% in the wake of Justice Van Devanter's resignation. Over the entire period, support averaged about 39%. Opposition to Court packing ranged from a low of 41% on 24 March to a high of 49% on 3 March. On average, about 46% of each sample indicated opposition to President Roosevelt's proposed legislation. And it is clear that, after a surge from an early push by FDR, the public support for restructuring the Court rapidly melted. Without extensive analysis of the original surveys, one cannot know with certainty, but I surmise from the patterns that most of the ups and downs in Roosevelt's support probably resulted from movements to and from the realm of no opinion. Opposition remained relatively steady. The proportion with no opinion bobbed around a good deal, from a low of 9% on 3 March to a high of 25% on 19 May.

The Results

Table 1 presents the products of a multiple regression of support for Court packing on the independent variables, corrected for autocorrelation. In general, the results are crisp: five of the six coefficients reach statistical significance, the direction of signs makes sense, and the fit is tight ($R^2 = .88$). Figure 4, a plot of the actual observations of support for Court packing on the predicted values, illustrates the high quality of the statistical explanation; only a few substantial residuals materialize.

The coefficients in Table 1 dramatically demonstrate the considerable latitude the Supreme Court has in shaping the contours of public opinion. President Roosevelt's efforts to persuade the Democratic party and the public of the plan's urgency, on 4 March at the victory dinner and over the air on 8 March, did make a statistically significant—if substantively marginal—impact on support for his bill. In the short run, at least, FDR's speeches boosted support by nearly 3% ($b_1 = 2.7$). Nevertheless, in comparison to the Supreme Court's actions, the president's campaign made only a modest dent in public opinion.

Somewhat surprisingly, the overruling of *Adkins v. Children's Hospital* in *West Coast Hotel v. Parrish* on 29 March made little or no difference in the pattern of public support ($b_2 = -1.2$; $t = -.6$). In the scholarly literature and the textbooks,

Figure 3. Support for Court Packing

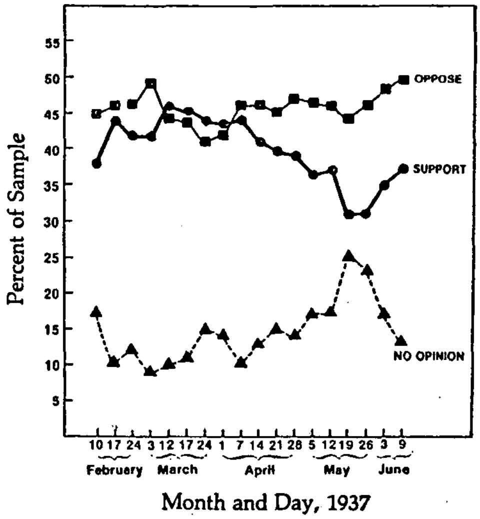


Figure 4. Support for Court Packing: Actual vs. Predicted

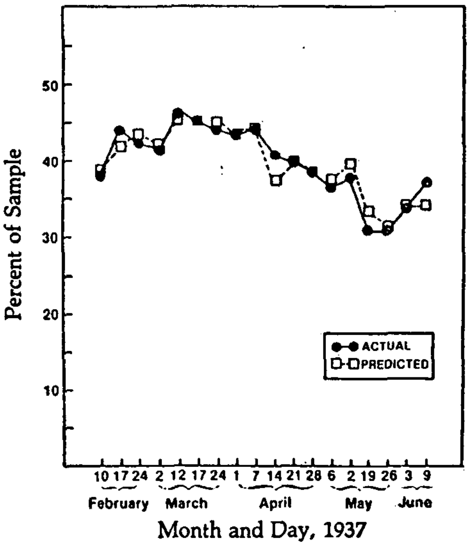


Table 1. Support for Court Packing

Independent Variable	b	t-Ratio	Level of Significance[a]
Intercept	38.97	16.35	.0001
Political events			
FDR's speeches	2.67	1.73	.0500
Overruling of *Adkins*	-1.16	- .64	n.s.*
Upholding of Wagner Act	-4.31	-2.48	.0150
Van Devanter's resignation	-5.63	-4.18	.0010
Communications			
Change in media coverage	- .04	-1.68	.0500
Direction of media coverage	.08	1.43	.0750

Note: $R^2 = .88$; $df = 11$; $N = 18$.

[a] Two-tailed test of significance.

*Statistically significant at .10 or better.

of course, *Parrish* marks a watershed in constitutional development; but after fits and starts in the Court's hospitality toward state regulation of business, it is doubtful whether many observers saw the decision that way. Moreover, because the Supreme Court in *Parrish* dealt with a state law, the connection between the decision and the New Deal was only indirect. This is so even though the overruling of *Adkins* signaled a retreat and commentators rank it as important as *NLRB v. Jones and Laughlin Steel.* It expressed a change in attitude, and that was apparently too subtle for the public to detect; for we know that few members of the relevant political elites actually read the Court's opinions or make themselves familiar with their contents. And, clearly, even a smaller segment of the mass public monitors as much as the general direction of the Court's jurisprudence, let alone specific decisions. It is probably too much to expect the public to react sharply to subtle shifts in public policy.

Upholding the Wagner Act, a contemporary linchpin of the New Deal and salient because of the recent sit-down strikes, helped enormously in building opposition to the president's measure. As a result of *NLRB v. Jones and Laughlin Steel,* FDR and his allies lost more than 4% of the public ($b_3 = -4.3$; $t = -2.3$). In the short run, the president's loyalists tried to shore up support and portrayed the decision on the Wagner Act as a temporary, strategic retreat on the part of the justices. That approach obviously did not work. Key political actors, inside and outside the halls of Congress, who understood the decision could readily see the radical departure from the narrow conception of interstate commerce dominant on the Court during the 1930s that Chief Justice Hughes's opinion implied. These results suggest that elite views diffused among the general public in the well-known two-step flow of information and opinions.

Justice Van Devanter's resignation—expected all along—sent the enemies of the Supreme Court into a tailspin. The announcement of Van Devanter's exit alone subtracted more than 5% ($b_4 = -5.6$) from the segment supportive of restructuring the Court. With that resignation, the prospect of a new justice, and the streak of 5–4 decisions, it must have seemed clear to many that FDR had a chance to have a sympathetic if small coalition on the Court. Van Devanter's departure, on the heels of *NLRB v. Jones and Laughlin Steel,* made it difficult for even the most loyal New Dealers to demonstrate the need for radical judicial reform.

Two crucial events—*Jones and Laughlin Steel* and Justice Van Devanter's resignation—spelled doom for FDR's bill to enlarge the Court and pack it with justices favorable to the New Deal. Between the two decisions, the Supreme Court decreased support for President Roosevelt's proposal by nearly 10%. Commentators and the justices themselves speak of the Supreme Court as relatively helpless in the arena of public opinion; but these results suggest the contrary. Planned or not, the Court's actions played a dramatic role in defeating the president. Chief Justice Hughes's active participation and superb timing in supplying information and surfacing in the news on occasion during the crisis indicate a great deal of political savvy (Mason 1956). Hughes, of course, had served in many political roles, and he behaved in a manner we might expect from an experienced politician.

The mass media's treatment of the Supreme Court made an appreciable impact on the shape of public sentiment for restructuring. For each 12% increase in favorable coverage of the plan in the *New York Times,* support for Court packing increased 1% (b_6 = .08). Since on average 40% of the stories put FDR's plan in a positive light, the *New York Times'* treatment contributed about 3.2% to the coalition in support of Court packing. Change in media coverage, on the other hand, detracted from the plan's support: for each increase of 25 stories, the segment in favor of the proposal declined by about 1% (b_7 = −.4). Thus, as I suggested, the bright light of increased scrutiny actually hurt its chances. This finding dovetails nicely with results I have reported elsewhere: in a study of public support for the Court in 1966–84, increases in media coverage of the Court increased support for the justices. The opponents of FDR managed to expand the scope of conflict to include all manner of people and groups heretofore unorganized. Extended media attention to the issue aided and abetted that mobilization. The intensity and direction of media coverage apparently exercised an equal influence on support for Court packing—after standardization, for media attention, B_6 = −.20; for the balance of coverage, B_7 = .19. In sum, then, media coverage made a difference in the battle over public opinion.

Conclusions

Public support for Court packing varied considerably over the space of 18 weeks and defies a simple account; but I have tried to trace meaningful shifts and to frame and evaluate explanations of the dynamics of public opinion on this issue. How can we explain the ebb and flow of sentiment for the president's plan (and presumably against the Supreme Court)? The rival hypotheses included political events—FDR's speeches, the Court's decisions in *Parrish* and *Jones and Laughlin Steel,* and Van Devanter's resignation—and the salience and direction of coverage in the mass media. Political events and mass communication both shaped public attitudes on the issue in the aggregate. Even more important, for present purposes, the actions taken by the Court and the justices played a crucial part. Historians will no doubt continue to argue about the political motivations behind and the timing of the Supreme Court's decisions; but regardless of the intentions of the participants, the Supreme Court's behavior made a difference. And to the outside observer, the Supreme Court did not seem to be a passive party, sitting idly by on the sidelines.

Scholarship on the origins and defeat of FDR's plan to restructure the federal judiciary, like so much else written on the Supreme Court, often subscribes to a mythical view of public attitudes toward the high bench (for an old but classic example, see Lerner 1937). Thus, for example, a recent student of the skirmish states that "for various reasons, Americans generally have regarded the judicial process as somehow outside or above politics" (Nelson 1986, 14). Trying to fathom FDR's failure in the arena of public opinion, a distinguished historian of the period has concluded that the "greater the insecurity of the times, the more people cling to the few institutions that seem unchangeable" (Leuchtenberg 1963, 235-36). Perhaps members of the public hold the Supreme Court in reverence and that is why FDR's proposal went up in flames. Yet the evidence accumulated over the years goes against that notion of the relationship between the public and the Court. I prefer, instead, a much more straightforward account: the Supreme Court outmaneuvered the president. Through a series of shrewd moves, the Court put President Roosevelt in the position of arguing for a radical reform on the slimmest of justifications.

Those moves, of course, spelled an important jurisprudential retreat on the part of the Court. President Roosevelt, in essence offered the Supreme Court a choice between substantive policy and structural integrity. The Court wisely chose to give up on the substantive issues and preserve its structural integrity. And public opinion, as we have seen, supported the Court in this choice. Perhaps one lesson we can extract from these results is that if the justices wish to gain public support in battles with the popularly elected branches, they must first assess whether the public stands with the president and Congress on the substantive issue; and, if so, the Supreme Court can preserve its institutional integrity by retreating on the issue. The results I have reported here suggest that the public will support existing constitutional arrangements or, at a minimum, existing arrangements as they pertain to the judiciary, so long as these arrangements are the sole issue. It is not at all clear what the public would have done if the Court had insisted on continuing its opposition to the New Deal. Congress and the president cannot successfully diminish the Supreme Court's structural integrity unless they have a major substantive dispute with the Court on issues to which the public has a strong commitment. Even then, we do not know whether the public would choose structural integrity or substantive policy. We do know that even an overwhelmingly popular public official cannot attack the structural integrity of the Court without the benefit of a very important policy issue for which he or she has overwhelming public support.

The research reported here raises many related questions. Did the political events in the spring of 1937 exert a differential impact across strata in the population? It is quite plausible, for example, that Democrats and union members responded differently to the issue than did Republicans and white-collar workers. Did FDR's fireside chat make a bigger mark among his loyalists and in parts of the nation more sympathetic to the New Deal than among others? Observations over time can help us to come to grips with that sort of question. In the cross-sectional case, can we identify the social, political, and economic bases of support for and opposition to the Supreme Court? The answers to these and other questions have important implications for the study of public opinion and the Supreme Court.

Notes

For some of the data presented here, I appreciate the assistance of the staff of the Roper Opinion Research Center. Michael S. Lewis-Beck advised on some statistical issues, and Peverill Squire made thoughtful comments on an earlier draft.

1. In this section and the next, I have relied heavily on the historical accounts cited here. To smooth the narration, I have refrained from making references to particular books or articles along the way. In his three articles, Leuchtenberg (1966, 1969, 1985) provides an especially good collection of sources on Court packing.

2. If a story, letter to the editor, or editorial praised, endorsed, or sympathized with President Roosevelt's approach to the Court, I coded it as *anti–Supreme Court.* If, on the other hand, coverage placed the Court in a positive light or reported criticism of FDR's proposal, I designated it as *pro–Supreme Court.* Letters to the editor and editorials created few problems. Straight reports proved more difficult. If a story reported on someone who criticized the proposed legislation, I classified it *pro-Court.* Since few people or institutions adopted a neutral stance, I had relatively little trouble in making judgments.

3. Naturally I would like to have data on the balance of coverage from a wide variety of newspapers. That would have entailed far more greater resources than I had at my disposal. Since the data from the *New York Times* mask the variations among newspapers across the nation and attenuate the relationship between the direction of coverage and changes in public opinion, I have undoubtedly underestimated the impact of the mass media on support for Court packing.

4. For simple interrupted time-series analysis, the multiple regression equation takes on the form

$$Y_t = b_0 + b_1X_{1t} + b_2X_{2t} + b_3X_{3t} + e_t$$

where Y_t = N time-series observations on the dependent variable; X_{1t} = a dummy variable counter for time from 1 to N; X_{2t} = a dichotomous dummy variable scored 0 for observations before the event and 1 for observations after; X_{3t} = a dummy variable counter of time scored 0 for observations before the event and 1, 2, 3, . . . for observations after the event (see Lewis-Beck 1986, 215).

5. More specifically, I used the AUTOREG procedure in SAS. It first estimates the model with ordinary least squares and then obtains estimates of the autoregressive parameters. Accordingly, the program transforms the original data by the appropriate autoregressive model. Finally, after making adjustments, SAS reestimates the ordinary least-squares regression. If all goes well, the procedure should eliminate the problems caused by autocorrelation. (See SAS Institute [1982, 187-202].) For this equation, I encountered no significant autoregressive parameters [AR(1) = .17; AR(2) = .00]. Thus, the results reported in Table 1 are the product of ordinary least squares.

6. In the first half of the series, the Gallup Poll asked, "Are you in favor of President Roosevelt's proposal regarding the Supreme Court?" Respondents could answer *yes, no,* or *no opinion.* Later, for inexplicable reasons, Gallup shifted the wording slightly to "Should Congress pass the president's Supreme Court plan?" Changes in wording are always cause for worry, but my analysis indicates that this particular alteration made little difference. First of all, relationships between support for Court packing and various independent variables such as partisanship, attitude toward FDR, and demographics did not change. Second, in the context of the fully specified model, a dummy variable for question wording does not reach statistical significance. Third, the substance of the question did not change, even though the wording did. Before and after the change, respondents had to make a decision on FDR's proposed law; Court packing constituted the stimulus in both versions.

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THE CORE OF THE CONSTITUTION

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It is often argued that the United States Constitution was designed so as to create a stable *political order. Yet in the literature on the formal theory of democracy, there has been very little examination of constitutional provisions for their stability-inducing properties. In this paper we demonstrate that bicameralism and the executive veto tend to create stability, that the legislative override of the executive veto tends to undermine this stability, and that the interaction of bicameralism and the executive veto is likely to produce stable outcomes despite the destabilizing impact of the veto override.*

Designing a constitution that would ensure stability in the laws was a major concern of some of the central participants in the Constitutional Convention. In his defense of the resulting document in *The Federalist Papers,* for example, James Madison argued that the proposed Senate, with its slow turnover and insulation from the public, was needed to help avoid instability: "The mutability in the public councils arising from a rapid succession of new members, however qualified they may be, points out, in the strongest manner, the necessity of some stable institution in the government" (*Federalist* no. 62 [p. 380]). In Madison's view this "mutability in the public councils" cost greatly: "To trace the mischievous effects of a mutable government would fill a volume" (p. 380).

The costs of instability were both external and internal in nature. Externally, instability causes the nation to forfeit "the respect and confidence of other nations" (p. 380): "Every nation . . . whose affairs betray a want of wisdom and stability, may calculate on every loss which can be sustained from the more systematic policy of its wiser neighbors" (p. 381). Internally, the consequences of instability were even worse: "The internal effects of a mutable policy are still more calamitous. It poisons the blessings of liberty itself. It will be of little avail to the people that the laws are made by men of their own choice if the laws be so voluminous that they cannot be read, or so incoherent that they cannot be understood; if they be repealed or revised before they are promulgated, or undergo such incessant changes that no man, who knows what the law is today, can guess what it will be tomorrow" (p. 381). This instability had distributional consequences in the society: "Another effect of public instability is the unreasonable advantage it gives to the sagacious, the enterprising, and the moneyed few over the industrious and uninformed mass of the people. Every new regulation concerning commerce or revenue, or in any manner affecting the value of the different species of property, presents a new harvest to those who watch the change, and can trace its consequences" (p. 381). Com-

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merce could also expect to suffer from unstable government: "The want of confidence in the public councils damps every useful undertaking, the success and profit of which may depend on a continuance of existing arrangements. What prudent merchant will hazard his fortunes in any new branch of commerce when he knows not but that his plans may be rendered unlawful before they can be executed?" (p. 381).

The Senate, with its members' six-year terms, their election by state legislatures, and the chamber's slow rate of turnover, could be expected to help avoid this instability. The independently elected president with his veto power was also expected to help. Hamilton argued in *Federalist* number 73 about the consequences of the veto: "It may perhaps be said that the power of preventing bad laws includes that of preventing good ones; and may be used to the one purpose as well as to the other. But this objection will have little weight with those who can properly estimate the mischiefs of that inconstancy and mutability in the laws, which form the greatest blemish in the character of genius of our governments. They will consider every institution calculated to restrain the excess of lawmaking, and to keep things in the same state in which they happen to be at any given period as much more likely to do good than harm; because it is favorable to greater stability in the system of legislation" (pp. 443–44).

Rapid turnover of legislators was one cause of instability in the laws, and Madison was also concerned about "the propensity of all single and numerous assemblies to yield to the impulse of sudden and violent passions, and to be seduced by factious leaders into intemperate and pernicious resolutions" (*Federalist* no. 62 [p. 379]). But there is reason to think that Madison's views on the instability of republics were also buttressed by a deeper theoretical understanding of the instability of majority rule. Theorists of democracy have long been troubled by the fact that a key element of most democratic systems—majority rule—has some undesirable properties. Among these is the fact that for any option that might be chosen by a majority of voters there is usually some other option, preferred by a different majority, which can upset it. Recognition of this problem can be traced back at least two hundred years, to Condorcet's seminal *Essai* (1785) and McGrath (1983, chap. 3) points out that Madison was well acquainted with Condorcet's work; Madison even wrote a review (now unfortunately lost) of the *Essai* and its arguments.

For Madison, then, there were theoretical as well as empirical reasons for designing a government that did not depend on simple majority rule. The Constitutional Convention's chosen design involved, in part, the separation of powers. That the separation of powers has indeed played a major role in creating a stable republic has been argued by many generations of historians and political scientists.

Yet while the formal literature on the theory of democracy has explored the nature of majority-rule instability in great detail—see Cohen 1979; McKelvey 1976, 1979; Miller 1980; Schofield 1978; and Shepsle and Weingast 1984, for example—there has been almost no study of whether a separation-of-powers system has the stability-inducing properties that Madison and Hamilton desired. As far as we can find, only Cox and McKelvey 1984 and Cox 1980 even address the relationship between bicameralism and stability. The former informally describes general conditions for stability in multicameral systems (p. 77). The latter argues that dividing a legislature into two chambers whose members have completely separate Pareto sets will ensure stability in two dimensions (see p. 10). Cox further conjectures that adding more policy dimensions can destabilize the situation: it appears that as many veto groups (e.g., chambers or committees) as dimensions are needed to guarantee stability.

Our work is in some ways more developed and in other ways more limited that that of Cox and McKelvey. For the set of constitutional provisions governing the legislative process—bicameralism, the executive veto, and the legislative veto override—we provide tools and formal results for characterizing the existence, shape, and location of any set of stable points. However, we assume that each legislator and the executive has "Euclidian preferences," that is, a most-preferred point and circular indifference curves in the issue space. We also restrict our analysis to the case of two dimensions. If legislators and the executive are considering an issue involving no more than two dimensions, the constitutional rules we are considering may well create stability. For more complex issues, the constitutional structure may be an insufficient guarantor of stability, and additional institutional features may be required.

Pure Majority Rule and the Bicameral Game

In a social institution, one alternative *dominates* another alternative when there is a set of individuals whose members all prefer the first alternative to the second and who can, given the rules of the game, enforce the first alternative over the second. The *core* of this institution is the set of undominated alternatives. In a pure majority-rule legislature, the core is the alternative that can gain a majority of votes against any other in a two-way vote. When there are three or more legislators and two or more dimensions by which options can be evaluated, a simple majority-rule legislature—playing what we will call a "unicameral game"—generally lacks a core. That is, for each possible legislative choice there almost always exist other options that are majority preferred (McKelvey 1976, 1979).

For example, consider the illustration in Figure 1. If the six legislators vote by simple majority rule, legislators H_1, H_2, H_3, and S_3 all prefer alternative p to c: p lies inside the (circular) indifference curves through c of legislators H_1, H_2, H_3, and S_3. A different majority coalition prefers e to p (indifference curves—not drawn—through p of S_1, S_2, S_3, and H_3 all contain e). Yet another coalition (H_1, H_2, S_1, S_2) prefers c to e. No alternative can gain the support of a four-member majority coalition against all other alternatives.

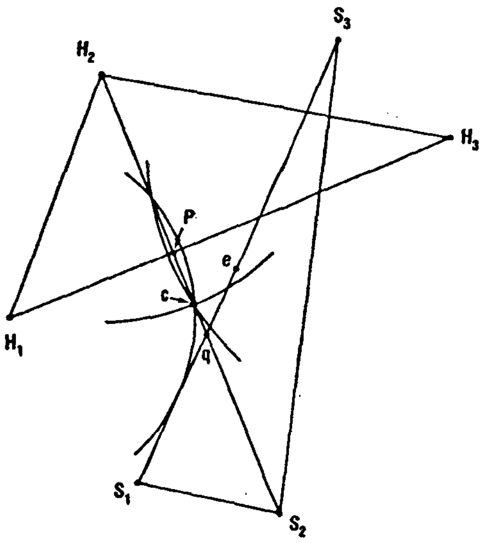
Various constitutional provisions can help overcome this generic instability of simple majority rule. Suppose H_1, H_2, and H_3 are all assigned to one chamber (the House), while S_1, S_2, and S_3 are all assigned to a different chamber (the Senate). For a bill to pass, it must gain the support of a *particular* four-person coalition—a *joint majority*—consisting of a majority of *each* chamber. These same six legislators no longer generate a majority-rule cycle. There is now a core that includes the point c: no joint majority will upset c in favor of some other point. The institution of bicameralism thus induces stability, given these six legislators' preferences.

The following section introduces theorems about when bicameralism has this stability-inducing effect. We also show how to find the shape of the bicameral core when it exists.

The Bicameral Game

Madison's analysis of bicameralism in *Federalist* numbers 62 and 63 lays out, in surprisingly accurate terms, the conditions under which bicameralism can avoid instability. The basic logic, he suggested in discussing the likelihood that the House

Figure 1. A Six-Person Legislature with a Bicameral Core but No Unicameral Core



and Senate would agree on undesirable policies, was that "the improbability of sinister combinations will be in proportion to the dissimilarity in the genius of the two bodies" (no. 62 [p. 379]). In our terms, when the ideal points of the members of the two chambers are sufficiently distant from each other, legislative outcomes will be stable: the bicameral game will have a core. Moreover, the outcomes will always lie within the legislative Pareto set: clearly undesirable policies are thereby avoided, as Madison desired.

The required separation between chambers was to be achieved by insulating the Senate from the popular passions to which the House would be exposed. If the Constitution was designed so as to induce in senators preferences substantially different from those induced in representatives, how great must these interchamber differences be for bicameralism to produce a core? To answer this question it is first useful to introduce several definitions and lemmas.

Lemmas about Bisectors

For the members of one chamber, draw the contract curve between the ideal points of each pair of members and extend each contract curve indefinitely in each direction. Some of these contract curves as extended (henceforth we refer to them simply as *contract curves*) will meet both of the following conditions:

1. The number of ideal points lying on the contract curve plus the number of ideal points to one side constitute a majority of the chamber's members
2. The number of ideal points lying on the contract curve plus the number of ideal points to the other side constitute a majority of the chamber's members

A chamber-member/chamber-member contract curve meeting both conditions is a *chamber bisector*. It is easily proved (Hammond and Miller 1987, 5, Lemma 1) that every chamber will have at least one chamber bisector.

If a contract curve is a chamber bisector, it is *attractive both ways*. That is, for any point not on the chamber bisector, there is at least one point on the chamber bisector that is majority preferred; see Davis, DeGroot, and Hinich 1972, where chamber bisectors are known as *medians*, or, in *n*-dimensional space, *median hyperplanes*. A simple geometric version of this assertion will help intuitive understanding:

LEMMA 1. *A chamber bisector is attractive both ways* (Davis, DeGroot, and Hinich 1972, Theorem 2).

Proof. The definition of *chamber bisector* implies that there will always be a majority of members on or to one side of the chamber bisector. It is therefore only necessary to prove that any point w on the *other* side of the chamber bisector from this majority can be beaten by some point on the bisector supported by this majority. In particular, consider the point x on the chamber bisector that is closest to w. (It will be the base of a perpendicular dropped to the bisector through w.) The point x will be closer to the ideal points of every legislator on the chamber bisector. It will also be closer to the ideal points of every legislator on the other side of the chamber bisector from w. Thus, the proposed majority will be willing to upset any point on the opposite side of the chamber bisector. The same argument applies to the analogous majority on or to the other side of the chamber bisector. Hence every point off the chamber bisector can be upset by some point on the chamber bisector. QED

There is nothing about a chamber bisector's properties that tells us what *is* in the core of the bicameral game. But Lemma 1 will be useful in telling us what points are *not* in the core.

Next, draw each House-member/Senate-member contract curve and extend the curve indefinitely. A joint majority consists of a majority of the House plus a majority of the Senate. When there is an odd number of members in each chamber, one or more of these cross-chamber contract curves will meet both of the following conditions:

1. The number of ideal points lying on the contract curve plus the number of ideal points to one side constitute a joint majority
2. The number of ideal points lying on the contract curve plus the number of ideal points to the other side constitute a joint majority

When there is an even number of members in one or both chambers, one or more of these cross-chamber contract curves will meet both of the following conditions:

3. The number of ideal points lying on the contract curve plus the number of ideal points to one side constitute a joint majority
4. The number of ideal points lying on the contract curve plus the number of ideal points to the other side constitute no fewer than one less than a majority in each chamber

A House-member/Senate-member contract curve meeting one of these pairs of conditions is a *bicameral bisector.* In Figure 1, the H_2S_2 contract curve is a bicameral bisector. As with a chamber bisector, it is easily proved that a bicameral bisector will always exist in a bicameral legislature.

If a House-member/Senate-Member contract curve is a bicameral bisector, it is *attractive one way* and may be, but is not necessarily, *attractive both ways.* If it is attractive both ways, for any point not on the bicameral bisector, there is a point on the bisector preferred by a joint majority. For an odd number of members in each chamber the following holds:

LEMMA 2. *If each chamber has an odd number of members, each bicameral bisector has a joint majority on or to each side and is thus attractive both ways.*

Proof of this lemma is almost identical to that of Lemma 1.

If one or both chambers has an even number of members, the bicameral bisectors will commonly have a joint majority only on or to one side—that is, Conditions (3) and (4) are usually met—and each bisector will be attractive only one way.

We can now state the most general conditions for a bicameral game to have a core:

THEOREM 1. *A point x is in the core* if and only if *no straight line through x leaves a majority of both chambers to one and the same side of that line.*

Proof consists of showing that a core exists when, for each majority in one chamber that wants to move in one direction from x, at least half of the other chamber will want to move in the opposite direction from x (see Appendix).

Theorem 1 defines the most general conditions under which a bicameral core will exist. But the theorem is not very useful for determining if a core exists with any particular preference profile: we would have to examine every possible x in the policy space to see if it meets both conditions. And finding one such point tells us little about the existence or location of any other such point.

However, identification of the chamber and bicameral bisectors leads directly to techniques for deducing when a core will exist and establishing what it looks like. Let us first look at the example in Figure 1. The House-Senate bicameral game has a core, which is that portion of the H_2S_2 bicameral bisector lying between House chamber bisector H_1H_3 and Senate chamber bisector S_1S_3; that is, the core is line segment pq. The reason? By Lemma 2, H_2S_2 is attractive both ways, so points not on H_2S_2 are not in any core. Furthermore, while a chamber bisector is attractive both ways when there is just a single chamber (Lemma 1), in a bicameral legislature it is attractive in a given direction only if a majority of the other chamber concurs. For example, points above H_1H_3 are attracted to H_1H_3 (from H_2's general direction) because all three members of the Senate would agree with H_1 and H_3 to move to H_1H_3. However, points below H_1H_3 are not necessarily attracted to H_1H_3 (from S_2's general direction) because a majority of the senators would not concur. A similar argument applies to points above and below S_1S_3. Points on H_2S_2 above H_1H_3 and below S_1S_3 are thus eliminated from any core. What remains is pq.

Note in this example that neither chamber has a core, nor does the unicameral legislature formed by combining the two chambers into one large chamber. Yet the bicameral game does have a core.

If one chamber has an even number of members, this increases the size of the core. Instead of having a single pivotal member at the end of a bicameral bisector, there are generally two bicameral bisectors leading to two pivotal members of the chamber with an even number of members. The *area* between these bicameral bisectors contains candidates for the core (see Hammond and Miller 1987, fig. 5 for an example).

What underlies the Figure 1 example is the fact that there is a set of points on a bicameral bisector that lies between all House chamber bisectors on the one hand and all Senate chamber bisectors on the other. With this observation, we now present a theorem about the existence of a core in bicameral games with one bicameral bisector:

THEOREM 2. *If (1) a bicameral legislature has only one bicameral bisector and (2) there is a point x on the bicameral bisector such that the chamber bisectors from one and only one chamber intersect at the bicameral bisector in each direction from x, then x is a core point.*

(Claim 2 in Cox 1980, p. 10 is implied by this theorem: nonoverlapping of chamber Pareto sets is sufficient for stability but not necessary.) This theorem applies to examples like Figure 1. In effect, for each x on pq, each House majority wants to move in one direction along pq and each Senate majority wants to move in the opposite direction along pq. Stalemate—stability—is the result. (See Appendix for formal proof.)

Bicameral games do not necessarily have a core. In general, the more the chambers resemble each other—the more overlapping their sets of ideal points—the less likely it is that bicameralism will create a core. While the authors of the Constitution desired the House and Senate to be substantially different from each other, it would seem for contemporary Congresses that the ideal points of the two chambers are rather similar, perhaps due to the impact of the 17th Amendment requiring the popular election of senators, perhaps due to the development of the party system.

As a result, there will usually be more than just the one or two bicameral bisectors we have analyzed so far. In these more complex cases, determining whether a core exists is eased by the following theorem:

THEOREM 3. *When there is an odd number of members in each chamber, there are three or more bicameral bisectors, and the bicameral bisectors do not all intersect at the same point, then there is* not *a core in a bicameral game.*

The proof is simply that since each bicameral bisector is attractive both ways (Lemma 2), each bisector attracts points from off itself, including points on other bicameral bisectors. This means that every point in the space is attracted by some other point. Hence there is no core. Since it seems unlikely that three or more bicameral bisectors will all intersect at precisely the same point, we pose the following:

CONJECTURE 1. *When each chamber has an odd number of members and there are three or more bicameral bisectors, the bicameral game usually has no core.*

Examples of cases in which one or both chambers has an even number of members (see Hammond and Miller 1987, Fig. 11) lead us to venture the following proposition:

CONJECTURE 2. *When there are three or more bicameral bisectors and there is an even number of members in one or both chambers, there is usually a core in a bicameral game.*

The Preservation of Prior Cores in Bicameral Games

Bicameral games can have a core when neither chamber has a core. The significance of this result might be undercut if bicameralism could at times destroy any core that the larger unicameral game (combining all members in one chamber) might have. We can demonstrate, though, that when a unicameral game does have a core, any bicameral game derived from the unicameral game by partitioning its members into two chambers (holding constant each legislator's ideal point) will still have a core. In fact, it is easily proved that the bicameral core will *include* the unicameral game's core:

THEOREM 4. *If a unicameral game has a core, then the bicameral core exists and includes the unicameral core.*

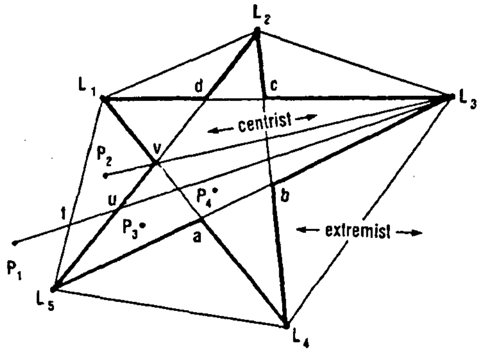
Proof. By contradiction: if the unicameral game has a core but the derived bicameral game has no core, this means that for every x in the issue space, there is a joint majority favoring some y to x. This implies that a majority of *all* legislators prefer the y to x. But this contradicts the condition that the unicameral game has a core. Hence, if the unicameral game has a core, the bicameral game must have a core. QED

This theorem means that bicameralism cannot be used as an institutional device for destabilizing simple majority rule in those cases in which simple majority rule produces stable outcomes. However, since bicameralism can increase the size of the core when there is a simple-majority-rule core, bicameralism can allow a different outcome than would occur due to simple majority rule.

The Executive-Veto Game

A bicameral game is not guaranteed to have a core. However, bicameralism is not the only constitutional provision that

Figure 2. A Five-Person Unicameral Legislature Facing a Chief Executive at Four Different Locations



can induce stability. We have already cited Hamilton's comment in *Federalist* number 73 that the executive veto would help maintain stability. It might be argued, on the basis of the historical record, that the veto is of little consequence in relations between president and Congress since it is used so infrequently. However, Hamilton suggested in *Federalist* number 74 that the veto does not have to be used to be effective: "A power of this nature in the executive will often have a silent and unperceived, though forcible, operation. When men, engaged in unjustifiable pursuits, are aware that obstructions may come from a quarter which they cannot control, they will often be restrained by the bare apprehension of opposition from doing what they would with eagerness rush into if no such external impediments were to be feared" (p. 446). If Hamilton was right, then we can expect the veto power to induce a core in an executive-veto game even if it is seldom exercised.

Since a simple veto allows the executive to veto any move from the executive's ideal point, that ideal point must be a part of a core. No further proof about this "executive veto game" is required to assert:

THEOREM 5. *The executive-veto game has a core that includes the executive's ideal point.*

We will next demonstrate that the core of the executive-veto game is not necessarily restricted to the executive's ideal point.

The Unicameral Executive-Veto Game

Here we explore executive veto games between an executive and a single legislative chamber. In Figure 2 the executive is located at any of the points labeled *P* and faces a five-person chamber. There are five chamber bisectors (L_1L_3, L_3L_5, L_5L_2, L_2L_4, and L_4L_1) which together enclose a five-pointed star defined by the $L_1L_3L_5L_2$-L_4L_1 sequence of points. Label an executive ideal point lying outside the star—outside the space completely bounded by chamber bisectors—as *extremist* and one lying on or inside the star—on or inside a space bounded on every side by chamber bisectors—as *centrist.*

If the executive's ideal point is centrist, the following is easily proved:

THEOREM 6. If the chamber has no core and if the executive has centrist preferences, the core of a unicameral executive-veto game contains only the executive's ideal point.

For example, in Figure 2 consider the executive ideal point at P_3. We treat the executive as a separate chamber. There are three "bicameral bisectors" (not drawn)—P_3L_2, P_3L_3, and P_3L_5. Since each bicameral bisector is attractive both ways, the executive can use some legislative coalition to upset every point in the space with an option closer to his or her own ideal point; the only point that cannot be upset is the ideal point. For similar reasons, an executive ideal point at P_4 is the only point in the core.

If the executive has extreme preferences—at P_1 or P_2 outside the star—the bicameral bisectors are P_1L_3 and P_2L_3

respectively. With P_1, the first chamber bisector that crosses the P_1L_3 bicameral bisector is L_2L_5; the point of intersection is u. The L_2L_5 chamber bisector attracts points from its right (the executive concurs) but not from the left (the executive would veto). Hence the core is P_1u. With the executive ideal point at P_2, the core is P_2v.

The Bicameral Executive-Veto Game

The following theorem helps identify the core of a bicameral executive-veto game:

THEOREM 7. *The bicameral executive-veto core includes (but is not restricted to) the House executive-veto core, the Senate executive-veto core, and the House-Senate bicameral core when it exists.*

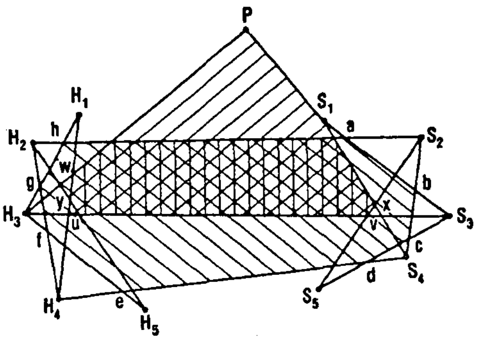
Proof consists of showing that if any two of the three institutions have a "bicameral" core, adding a third institution does not change the first two institutions' joint preference for points in their core over points not in their core. Any "bicameral" core cannot somehow be overruled just because some third institution prefers otherwise.

Figure 3 shows a five-member House and five-member Senate, with executive at P. The core of the bicameral House-Senate game is the uv segment of the H_3S_3 bicameral bisector. The executive-House core is the Pw segment of the PH_3 "bicameral bisector"; the executive-Senate core is the PS_1 segment of the PS_1 "bicameral bisector." The resulting bicameral executive-veto core, encompassed by the $PS_1xvuywP$ lines, is shaded by lines sloping down to the left.

The Veto-Override Game

A unicameral executive-veto game—an executive facing a single chamber—is guaranteed to be stable. A second legislative chamber is thus unnecessary for stability. The legislature's authority to override the executive's veto does resurrect the specter of instability. However, in *Federalist* number 73 Hamilton defends the override provision with a probabilistic argument about the inability of a *bicameral* legislature to override the veto. As he remarked, "It is to be hoped that it will not often happen that improper views will govern so large a proportion as two thirds of both branches of the legislature at the same time; and this, too, in defiance of the counterpoising weight of the executive. It is at any rate far less probable that this should be the case than that such views should taint the resolutions and conduct of a bare majority" (p. 446). We will argue that Hamilton's intuitions were largely correct. The stability induced by the veto power is undermined by the veto override, but the requirement of a *dual* override makes it likely that there will be some set of points that cannot be upset.

Figure 3. A Bicameral Legislature Facing a Chief Executive



A result of Greenberg (1979) plays an essential role in our analysis. Define D as the number of policy dimensions under consideration, L as the total number of members in the (unicameral) legislature, m as a bare majority of L, and k as the number of votes required to upset x in favor of some y ($m \leqslant k \leqslant L$). A *chamber k-core* is a set of points such that (1) for each y not in the set, there is some x in the set that is preferred by k or more members, and (2) no point in the set is preferred by k or more members to some other point in the set. A chamber k-core is, in effect, a generalization of a core for $k \geqslant m$.

We can now state

LEMMA 3. *If $k > [D/(D + 1)]L$, a unicameral game has a chamber k-core* (Greenberg 1979, Theorem 2).

When there are two dimensions ($D = 2$), for example, a chamber k-core is guaranteed to exist when $k > (2/3)L$. Assuming $k > m$, the following are easily shown:

LEMMA 4. *If a chamber has a majority rule core, it necessarily has a chamber k-core.*

(If a chamber has a chamber k-core, though, it does not necessarily have a majority rule core.)

LEMMA 5. *If a chamber has no k-core, it has no majority-rule core.*

Now define a *chamber k-sector* as a chamber-member/chamber-member contract curve that has k members on or to one side and the remaining members on the other side. Assuming at least k votes are needed to approve a motion, whenever $k > m$, the following is obvious:

LEMMA 6. *A chamber k-sector is attractive only one way.*

In Figure 2, there are four chamber k-sectors, L_1L_3, L_2L_4, L_3L_5, and L_4L_1. Assuming $k = 4$, together they define the chamber 4-core: it is the pentagon, *abcdva*, internal to the five-pointed star fixed by the five legislators' ideal points.

For a bicameral legislature the analogue of a chamber k-core is a *bicameral k-core*. It is a set of points that no coalition of k_H House members plus k_S Senate members can upset. To find a bicameral k-core, define a *bicameral k-sector* as a House-member/Senate-member contract curve with k_H House members and k_S Senate members on or to one side of the line. Assuming k members in each chamber are needed to approve a motion, it is obvious that

LEMMA 7. *A bicameral k-sector is attractive only one way.*

In the bicameral legislature in Figure 3, with $k_H = k_S = 4$, the bicameral 4-core is the *abcdefgha* region (shaded by lines sloping down to the right) circumscribed by the bicameral 4-sectors (H_2S_2 and H_4S_4) and the chamber 4-sectors (H_1H_3, H_2H_4, and H_3H_5 for the House and S_1S_3, S_2S_4, and S_3S_5 for the Senate). This bicameral 4-core contains points that no coalition of four or more House members plus four or more Senate members can upset.

Now assume that k is the number of members required to override a veto. Call a chamber majority with at least k members an *override majority* (also called a *k-majority*). Similarly, a *joint override majority* (a *joint k-majority*) has at least k_H members in the House and at least k_S members in the Senate. For simplicity in the following analysis we assume chambers of equal (and odd) size and $k_H = k_S = k$.

Given these preliminaries, what conditions are necessary for a core to exist in a veto override game? There are three different cases to examine, whether we are considering a unicameral or bicameral legislature.

For the first case, assume that a k-core (unicameral or bicameral) does not exist; if $k \leqslant (2/3)L$, for example, a k-core will not necessarily exist. If no k-core exists, there can be no veto-override core. The reason is simple. Since there is no k-core, by definition this means that for any x in the executive-veto core (which always exists) there is always some override majority (or joint override majority) that can upset x in favor of some y, another override majority that can upset the y in favor of some z, and so forth. In effect, the legislature can eliminate any point in the executive-veto core but, with no k-core, is incapable of replacing the executive-veto core with any points of its own. Hence we can state

THEOREM 8. *If there is no legislative k-core, there exists no veto-override core.*

For the second case, assume that there is a legislative k-core but that the executive-veto core (which always exists) does not intersect it. In this case, too, there will be no veto-override core. The reason? The executive-veto core always attracts points lying outside it. When the core of the executive-veto game does not intersect the k-core, this means that there is an executive-led coalition (containing the executive plus a legislative majority or joint majority) that can upset any point in the k-core. But since the k-core is also attractive (due to the bicameral and/or chamber k-sectors that bound it), any point in the executive-veto core can be overridden by some override majority (or joint override majority) preferring a point in the k-core. Outcomes thus bounce back and forth between the executive-veto core and the legislative k-core. So when the executive-veto core does not intersect the legislative k-core, the veto-override game has no core.

For the third case, assume that the executive veto core *does* intersect the legislative k-core. In this case the veto-override game has a veto-override core, which is the area of intersection of the executive-veto core and the legislative k-core. Consider some point x in this intersection. Since x is in the executive-veto core, no executive-led coalition can upset x. But since x is also in the legislative k-core, there is no legislative override majority that can upset x in favor of something else. Hence x is in the veto-override core. The following theorem thus holds:

THEOREM 9. *For a veto-override game (unicameral or bicameral), there is a veto-override core if and only if (1) the legislature has a k-core, and (2) the core of the executive-veto game intersects this k-core.*

Note an immediate corollary of Theorem 9:

THEOREM 10. *If unanimity is required for the legislature to override, a veto-override game always has a core.*

The reason is that the legislature's entire Pareto set becomes the k-core here, and an executive-veto core always intersects this Pareto set.

We now illustrate these various points.

Unicameral Veto-Override Games

Define an executive's ideal point as *k-centrist* if it lies in a chamber k-core. Otherwise, the executive's ideal point is *k-extremist.*

Now consider a five-person legislature with no core, as in Figure 2. Assume k = 4. As previously noted, the legislative 4-core here is the internal pentagon, *abcdva.* With a k-extremist ideal point at P_1, the executive-veto core is the P_1u segment of the P_1L_3 bicameral bisector. Since the executive-veto core does not intersect the legislative 4-core, there is no veto-override core: the executive-veto core attracts points from the legislative 4-core, and vice versa. An executive ideal point at P_3 is also unstable: P_3 is the unique executive-veto core, but it does not intersect the legislative 4-core, hence there is no veto-override core. However, the executive with preferences at P_2 generates an executive-veto core, P_2v, which intersects the legislative 4-core at v. Since v is in the executive-veto core, there is no executive-led coalition that will upset it. Since v lies in the 4-core, there is no four-legislator override coalition that can upset it. Hence v is the unique core to this veto-override game.

In general, if an executive has a k-centrist ideal point, the executive-veto core will always intersect the legislative k-core. Hence the following corollary to Theorem 9:

THEOREM 11. *If there exists a unicameral k-core and the executive has a k-centrist ideal point, the unicameral veto-override game has a core.*

Bicameral Veto-Override Games

Analysis of bicameral veto-override games is similar to that of unicameral veto-override games. Consider Figure 3. The core of the bicameral executive-veto game is the $PS_1xvuywP$ area shaded by diagonal lines sloping down to the left. Assuming $k = 4$, the bicameral 4-core is the *abcdefgha* region, as previously noted, shaded by lines sloping down to the right. The intersection of the bicameral executive-veto core and the bicameral 4-core is shaded by vertical lines. The joint-override majority eliminates points in the bicameral executive-veto core above the H_2S_2 bicameral 4-sector. The coalitions supporting the executive-veto core eliminate points in the bicameral 4-core below the H_3S_3 bicameral bisector as well as points lying to the left of the H_1H_4 and H_2H_5 chamber bisectors and the PH_3 bicameral bisector and to the right of the S_1S_4 and S_2S_5 chamber bisectors. But some portions of the executive-veto core intersect the bicameral 4-core. This region, shaded by the vertical lines, is the bicameral veto-override core.

The executive ideal point in Figure 3 lies *outside* the bicameral k-core. The following summarizes what happens if it is *inside:*

THEOREM 12. *If there is a bicameral k-core and the executive's ideal point lies inside it, the bicameral veto-override game has a core.*

The reason of course is that the executive-veto core here necessarily intersects the bicameral k-core. If there is no bicameral k-core, the bicameral veto-override game will haven no core. But the following theorem holds:

THEOREM 13. *If either chamber has a chamber k-core, (1) the bicameral legislature has a bicameral k-core, and (2) this bicameral k-core is not restricted to the chamber k-core.*

Finally, the following theorem may seem obvious but will help clarify the discussion in the next section:

THEOREM 14. *If a House-Senate bicameral game has a core, the veto-override game has a core that includes the bicameral core.*

Likelihood of a Bicameral Veto-Override Core

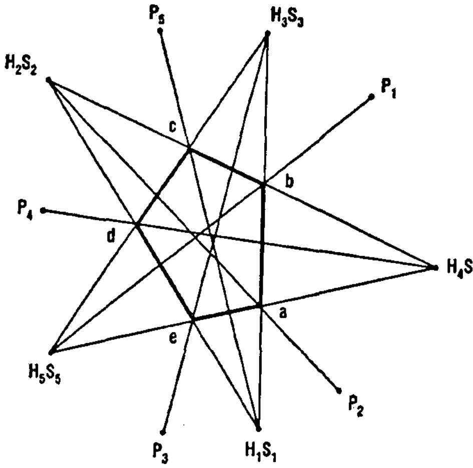
Does the Constitution, which requires a two-thirds vote in each chamber to override a presidential veto, guarantee stability? The answer is that the Constitution does not *guarantee* stability, but we will argue that stability is nonetheless very likely in the two-dimensional case.

Lemma 3 (Greenberg 1979) guarantees a chamber k-core if $k > (2/3)L$. With a 435-member House, a two-thirds override requires 290 votes. But $290/435 = 2/3$, hence no House k-core is guaranteed to exist. With a 100-member Senate, 67 votes are required for override; this is more than two-thirds. The Senate is thus guaranteed to have a k-core. Hence by Theorem 13 the Congress is guaranteed to have a bicameral *k*-core. But there is no guarantee that the executive-veto core will intersect this bicameral *k*-core.

Nonetheless, we argue that Hamilton's probabilistic argument is likely to be correct. Let us establish a framework for analyzing his argument. If there is sufficient separation between the ideal points of the House and Senate, as in Figure 3, there will be a core to the bicameral game (Theorem 2) and hence a core to the bicameral veto-override game (Theorem 14). At the other extreme, if there are no differences at all between House and Senate ideal points (in terms of paired House-Senate members), then we have a bicameral version of the unicameral veto-override game. In this case, bicameralism does not add any stability since one chamber is simply the mirror image of the other. The bicameral veto-override game's potential for stability here will be limited to conditions similar to those for unicameral veto-override games (Theorem 9).

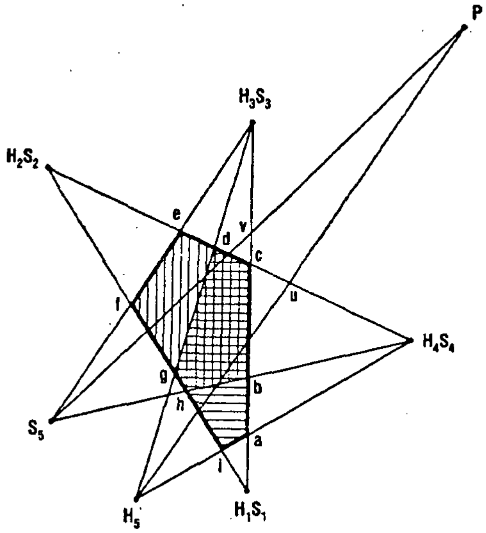
Now consider this case in which there is *no* difference between chambers (see Fig. 4). Each chamber has five members and all five House-Senate pairs perfectly coincide. Assume $k = 4$. An executive inside the bicameral 4-core (labeled *abcdea* and shown by the heavy outline) will generate an override core (Theorem 12). With an executive ideal point outside the bicameral 4-core, there are five rays on which an executive must lie for there to be a bicameral override core. These rays are the five executive-legislative bisectors—$P_1H_5S_5$, $P_2H_2S_2$, $P_3H_3S_3$, $P_4H_4S_4$, and $P_5H_1S_1$—that intersect the bicameral 4-core. Only if the executive ideal point lies on one of these five rays—which is very unlikely—will the resulting bicameral executive-veto core intersect the bicameral 4-core. An executive at P_1, for example, creates an executive veto core, P_1b, which intersects the bicameral 4-core at *b*. An executive ideal point not on one of these five bisectors will generate no override core since the executive-veto core will not intersect the bicameral *k*-core.

Figure 4. A Bicameral Legislature Facing a Chief Executive at Five Different Locations



However, if the members of even just *one* House-Senate pair of legislators do not have identical preferences, then there are numerous prospects for stability that did not previously exist. Consider the two five-person chambers in Figure 5. Members of four of the five pairs of legislators —H_1-S_1, H_2-S_2, H_3-S_3, and H_4-S_4—have identical preferences; only H_5 and S_5 differ. Assume $k = 4$. There is no core to the overall 10-legislator unicameral game because the required symmetry conditions are not met. Nor does either chamber have a core. The bicameral game between the two chambers has no core because there are more than two bicameral bisectors that do not all intersect at the same point (Theorem 3). Furthermore, there is no veto-override core for the president-House veto-override game since the House's executive-veto core, *Pu*, does not intersect the House 4-core, *acdgia* (horizontal shading). Nor is there a veto-override core for the president-Senate

Figure 5. Intersection of Executive-Veto Core with Bicameral 4-core



veto-override game since the Senate's executive-veto core, *Pv*, does not intersect the Senate 4-core, *bcdefhb* (vertical shading). So if there is a core to the bicameral veto-override game here, it must be due to the *interaction* between bicameralism and the executive's veto.

And in fact, there is a core to this bicameral veto-override game. By Theorem 9, a bicameral override core exists if the bicameral 4-core intersects the bicameral executive-veto core. The bicameral executive-veto core is the *PucvP* area. The bicameral 4-core is, in this case, simply the set union of the chamber 4-cores, producing the *acefia* pentagon, which has the heavy outline. The executive-veto core and the bicameral 4-core do intersect but only at *c*. Hence *c* is the core of this bicameral veto-override game. Note that *P* can be moved around substantially without eliminating *c* as the override core.

The modest divergence between the House and Senate, involving just H_5 and S_5, is thus sufficient to create possibilities for an override core that did not exist in Figure 4. If this divergence between H_5 and S_5 increases, the possibilities of a core increase. While space does not permit the presentation of further examples (see Hammond and Miller 1987, Figs. 21, 22), it appears that as additional cross-chamber pairs of legislators diverge, then the existence of a core becomes less and less sensitive to the location of the executive's ideal point. These increasing differences in legislative ideal points increase the size of both the bicameral *k*-core and the executive-veto core, making it increasingly likely that the two sets will intersect somewhere around the periphery of the bicameral *k*-core.

Note that this analysis has presumed the executive has an ideal point lying outside the bicameral *k*-core. If the executive's ideal point lies inside the bicameral *k*-core, recall that the bicameral veto-override game is guaranteed to have a core (Theorem 12).

Finally, it is important to note that unlike the simple bicameral game, large overlaps of the Pareto optimal sets for the two chambers do not necessarily diminish the likelihood of a bicameral veto-override core. It appears sufficient that the *individual legislators* are not identically matched across chambers. For this reason we feel safe in making the following conjecture:

CONJECTURE 3. *Bicameral veto-override games usually have a core.*

Summary of the Constitutional Order

Six different sets of constitutional provisions have now been examined: unicameral games (pure majority rule), bicameral games, unicameral executive-veto games, bicameral executive-veto games, unicameral veto-override games, and bicameral veto-override games. Considering the many complex relationships among games that have been presented, we now need to organize the results.

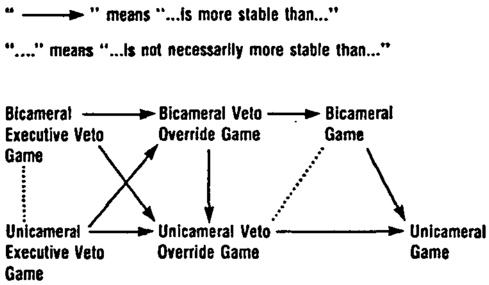
Define a *preference profile* as the set of preference orderings held by a group of legislators. We will say that constitution A *is more stable than* constitution B if two conditions hold: (1) if a given profile has a core under *B*, then that profile must have a core under *A*; and (2) there is some profile that has a core under *A* that does not have a core under *B*. Figure 6 summarizes the is-more-stable-than relationships among the six games. If the recipient of an arrow has a core for some preference profile, the originator of the arrow also has a core for that profile as well as for some other profiles. Since the is-more-stable-than relationship is transitive, we can deduce the relationship between each pair of games. For example, the bicameral executive-veto game is more stable than the bicameral veto-override game, the unicameral veto-override game, the bicameral game, and the unicameral game.

A *noncomparability* relationship is shown by a dotted line. Games are noncomparable in terms of stability for either of two reasons. In one kind of case, since both the bicameral and the unicameral executive-veto games are guaranteed to have cores, neither is more stable than the other. In the other kind of case, the nature of the relationship simply depends on the particular preference profile under consideration, as with unicameral veto-override games and bicameral games.

We should emphasize that the results upon which Figure 6 is based depend on our two key assumptions.

First, we have assumed that our two issue dimensions are equally important to every legislator, thereby producing circular indifference curves. The existence of executive-veto cores does not rely on an assumption about the shape of indifference curves. But the extent to which other results can be generalized when legislators and executives have noncircular indifference curves must be ascertained.

Figure 6. Stability Relationships among Six Different Legislative Games


" ——► " means "...is more stable than..."
"...." means "...is not necessarily more stable than..."
Bicameral Executive Veto Game
Bicameral Veto Override Game
Bicameral Game
Unicameral Executive Veto Game
Unicameral Veto Override Game
Unicameral Game

Second, we have presumed the existence of only two issue dimensions. Several of our results, to be sure, can be generalized to higher dimensions. But for a bicameral *k*-core to exist in higher dimensions requires a larger and larger value of *k* (see Lemma 3). Similarly, with a fixed value of *k*, as the number of dimensions increases, it seems reasonable to think that any bicameral *k*-core would get smaller and smaller, and there would be a decreasing likelihood that it would even exist. As a result, the executive-veto core (which will always exist) would be less and less likely to intersect a bicameral *k*-core (even assuming one does exist). This in turn means a decreasing likelihood that a veto-override core would exist.

Conclusions

We have examined the extent to which the Constitution creates a stable political order. To Madison, however, stability was not the only value the political order should have. In *Federalist* number 37, for example, he wrote:

> Among the difficulties encountered by the convention, a very important one must have lain in combining the requisite stability and energy in government with the inviolable attention due to *liberty* and to the *republican form*. . . . *Energy in government* is essential to that security against

> external and internal danger and to that prompt and salutary execution of the laws which enter into the very definition of good government. *Stability in government* is essential to national character and to the advantages annexed to it, as well as to that repose and confidence in the minds of the people, which are among the chief blessings of civil society. An irregular and mutable legislation is not more an evil in itself than it is odious to the people; and it may be pronounced with assurance that the people of this country, enlightened as they are with regard to the nature, and interested, as the great body of them are, in the effects of good government, will never be satisfied till some remedy be applied to the vicissitudes and uncertainties which characterize the State administrations (pp. 226–27, emphasis added)

From the Madisonian perspective, then, a properly designed Constitution requires not only stability-inducing features but several others as well.

We have little to say about the relationship of *stability in government* to *energy in government*, to the *republican form* of government, or to *liberty*. It is easily demonstrated, however, that any core (if it exists) to one of our games meets one test of "good" public policy, that of Pareto efficiency. A policy is Pareto efficient if there exists no other policy unanimously preferred to it. If there is a core to a bicameral game, for example, this means that for every alternative in the core there is no other policy preferred by a joint majority. And since there is no other policy preferred by a joint majority, it obviously follows that there is no other policy preferred unanimously, by all members of both chambers. From this we can conclude that points in the bicameral core are all Pareto efficient.

Given our focus on stability, one might plausibly raise the question, as have numerous critics of U.S. democracy, as to whether the constitutional system is, in some sense, *too* stable. Is it, for example, *unresponsive* to changes in the locations of the ideal points of legislators and the president?

A well-developed answer to this question requires another paper entirely. But our results may give some insight into the problem. It appears that the location of any bicameral veto-override core is quite responsive to changes in the location of presidential ideal points. On the other hand, this override core may be relatively resistant to large-scale changes in legislative preferences. In Figure 3, for example, as long as the locations of the president and legislators H_3 and S_1 do not change, many of the other legislators' ideal points can move about in many different ways without much impact on the shape, size, or location of the override core.

This kind of pattern may have been what was intended by those who drafted the Constitution. The (apparent) lack of responsiveness of the core to changes in legislative preferences certainly seems compatible with the concern of Madison and Hamilton about the "mutability in the laws." And it may not be stretching things too much to suggest that the responsiveness of the core to changes in presidential preferences is an essential component of *energy in government*. If these inferences and interpretations prove correct, then our results can be read as a formal demonstration that the Constitution should work, at least in part, as designers like Madison and Hamilton argued it would.

Appendix

Proof of Theorem 1

THEOREM 1. *A point x is in the core* if and only if *no straight line through x leaves a majority of both chambers to one and the same side of that line.*

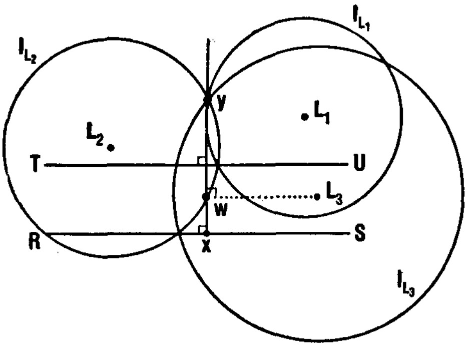
Proof. We first show that if x is in the core, then no straight line through x leaves a majority of both chambers to one and the same side of that line. It is a proof by negation, so we assume (see Fig. A-1) that there is some line RS through x that leaves a majority of both chambers to one and the same side of the line.

Consider legislator L_3 who is in the joint majority to one side of RS and who is closest to the RS line. Now construct a point w the same distance to the same side of the RS line, on a line perpendicular to RS. Now w must be closer to every legislator in that joint majority than is x. As a result, every legislator in the joint majority prefers w to x, and x cannot be in the core, contrary to assumption. Hence, the assumption that there is a joint majority to one and the same side of the line must be wrong.

We next demonstrate that if there is an x such that no line through x has a majority of both chambers to one and the same side of that line, then x is in the core. We will assume there is some y preferred by a joint majority to x and establish a contradiction.

Consider some point y, which, by assumption, is preferred by a joint majority to some x. This means that for a majority of the House and a majority of the Senate, x lies strictly outside their indifference curves through y. Now consider the diagram in Figure A-1. Draw a straight line from x to y. Draw two perpendiculars through the xy line, one through x—producing line RS—and one through a point that bisects xy—producing line TU. Consider any legislator, L_j, in this joint majority. The only way for L_j's indifference curve through y not to contain x is for L_j to lie above the TU perpendicular; see the indifference curves of L_1, L_2, and L_3 through y, for example. But if the ideal points of all legislators in this joint majority who prefer y to x (e.g., legislators like L_1 and L_2 but not L_3) lie above the TU line, then majorities of both the House and Senate necessarily also lie above the RS line through x. Hence there is a majority of both chambers whose ideal points are to one and the same side of a line through x, contrary to the assumption. Thus the assumption that x is not a core point must be wrong. QED.

Figure A-1. Establishing General Conditions for the Existence of a Core in a Bicameral Game



Proof of Theorem 2

In proving Theorem 2, two lemmas are useful:

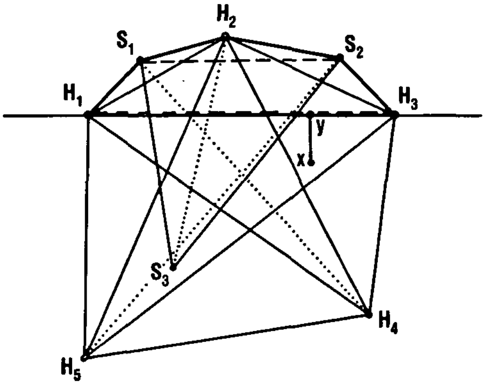
LEMMA A-1. *No point in the Pareto set of a joint majority of minimal size can be upset by some other point in this Pareto set due to the votes of the members of this joint majority.*

This lemma follows directly from the definition of the Pareto set of a group of actors, which is the set of alternatives from which they cannot agree to move.

LEMMA A-2. *Given some joint majority of minimal size, at least one of the contract curves connecting the ideal points of a given chamber's members in the joint majority will be a chamber bisector.*

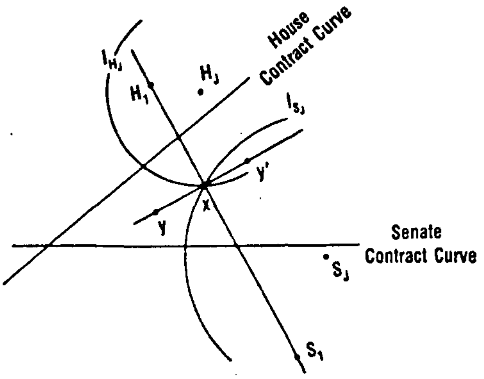
Proof. Draw the Pareto set of just the House members in the joint majority. Consider some point x outside this House Pareto set but inside the Pareto set of the entire House; see point x in Figure A-2, for example. This point is outside of the Pareto set for coalition H_1, H_2, and H_3. Establish some y on the nearest House

Figure A-2. At Least One Chamber-Member/Chamber-Member Contract Curve Will Be a Chamber Bisector



contract curve in the joint majority's Pareto set by dropping a perpendicular to the nearest House contract curve. Since by assumption the joint majority is of minimal size, only a bare majority of the House will prefer y on or in this House Pareto set to the x lying outside this House Pareto set. If this is not true for at least one House contract curve in this Pareto set, either (1) more than a bare House majority prefers y to x, which violates the assumption that the joint majority is of minimal size, or (2) less than a bare House majority prefers y to x, which violates the assumption that we are considering a joint majority. Since a bare House majority lies on or to one side of the House contract curve, a bare House majority must lie on or to the other side. Hence this House contract curve is a House chamber bisector. The same logic holds for the Senate contract curves in the minimal-size joint majority. QED

Figure A-3. Establishing the Existence and Location of a Core in a Bicameral Game



We can now prove

THEOREM 2. *If (1) a bicameral legislature has only one bicameral bisector and (2) there is a point x on the bicameral bisector such that the chamber bisectors from one and only one chamber intersect the bicameral bisector in each direction from x, then x is a core point.*

Proof. By negation: we assume Conditions (1) and (2), but assume that some point y beats x.

The House and Senate members at either end of (and defining) the bicameral bisector will be called H_1 and S_1 (see Figure A-3). Examining the joint coalition that prefers y to x, there are three mutually exclusive and collectively exhaustive cases to consider, each of which leads to a contradiction.

Case 1. Both H_1 and S_1 are members of the joint coalition preferring y to x.

The first combination is easily ruled out as a minimally sized joint majority that can upset x. Since the interests of H_1 and S_1 are diametrically opposed along H_1S_1, no minimal joint majority that includes both H_1 and S_1 can prefer some y to x. Some y off the H_1S_1 bicameral bisector would be rejected by both H_1 and S_1 in favor of some point on the bicameral bisector, while movement along the bicameral bisector necessarily leaves one one of them worse off. So the joint majority preferring y to x will necessarily lack either H_1 or S_1 or both.

Case 2. Neither H_1 nor S_1 are members of the joint coalition preferring y to x.

We next prove that no minimally sized joint majority lacking both H_1 and S_1 can exist that prefers some y to x. Draw a perpendicular to H_1S_1 through x. We will show that the indifference curves through x of a Senate majority cannot all cross the perpendicular from below and that the indifference curves through x of a House majority cannot all cross the perpendicular from above. This will mean that the $H^{-1}(x)$ win set of a House majority cannot possibly intersect the $S^{-1}(x)$ win set of a Senate majority. Recall that H_1S_1 is the only bicameral bisector. Hence, to the right of H_1S_1 there is, by definition, no more than one less than a majority of the Senate, while to the left of H_1S_1 there is no more than one less than a majority of the Senate. Since we have by assumption ruled S_1 (as well as H_1) out of the joint majority upsetting x in favor of y, the Senate members of the joint majority must come from *both* the right *and* the left of H_1S_1; no other way of mustering a Senate majority is possible. However, no indifference curve through x of any Senator to the right of H_1S_1 can possibly cross the perpendicular on the left of H_1S_1 (see the indifference curve of S_j, labeled I_{Sj}, for example). Similarly, no indifference curve of any House member to the right of H_1S_1 can possibly cross the perpendicular on the left of H_1S_1 (see the indifference curve of H_j—I_{Hj}—for example). Hence the $H^{-1}(x)$ and $S^{-1}(x)$ win sets cannot intersect to the left of H_1S_1. As a result, there can exist no joint majority that prefers some y to the left of H_1S_1. An identical argument demonstrates that there can exist no joint majority that upsets x in favor of some y' to the right of H_1S_1. Therefore, no joint majority including *neither* H_1 nor S_1 will prefer some y to the x.

Case 3. Either H_1 or S_1 are members of the joint coalition preferring y to x, but not both.

Assume without loss of generality that it includes only H_1. Hence a joint majority including H_1 that upsets x in favor of some y must include a majority of Senate members not involving S_1. Since there is no Senate majority that lies strictly on one side of the bicameral bisector, the joint majority including H_1 must include Senate members from both sides of the bicameral bisector.

But the Pareto set of the minimal-size joint majority just described—containing H_1 and senators (but not S_1) on both sides of the bicameral bisector—will encompass space on both sides of the bicameral bisector. Now consider the Pareto set of just the Senate members of this minimal-size joint majority. The boundaries of the Senate-majority Pareto set will cross the bicameral bisector at two places, at the top of the set (toward the H_1 end) and at the bottom of the set (toward the S_1 end). By Lemma A-2, one of these lines crossing the bicameral bisector will necessarily be a Senate chamber bisector. If it is not a Senate chamber bisector, either (1) the number of senators above the line is less than a majority, in which case x is not in fact upset by the joint majority—contrary to the assumption that x is upset, or else (2) the number of senators above the line is more than a bare majority, in which case we have more than a minimal Senate majority contributing to the upsetting of x —contrary to the assumption that we are dealing with a minimal-size joint majority.

Now, since we have assumed there is no bicameral core, point x must lie outside the Pareto set of the minimal-size joint majority (Lemma A-1), which necessarily means that it lies *below* the Senate chamber bisector just described, which crosses the bicameral bisector. But this means that a Senate chamber bisector crosses the bicameral bisector on the same side as the House majority. This in turn means that there are both House and Senate chamber bisectors intersecting the bicameral bisector on the same side of x. This violates Condition 2. Again, in Case 3, we arrive at a contradiction.

Since a contradiction is reached in all possible cases, it cannot be the case that x is not a core point. QED

Note

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TOCQUEVILLE'S CONSTITUTIONALISM

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For the ancient philosophers, constitutionalism *meant classifying regimes and constructing regimes to form virtuous citizens. In the modern world it generally means checks and balances, institutional mechanisms limiting the power of government and protecting private rights. In* Democracy in America *Tocqueville attempts to combine both views in his interpretation of the U.S. constitutional system. He employs the regime analysis of ancient constitutionalism to understand the new phenomenon of popular sovereignty and its potential for despotic control over the minds and characters of citizens. At the same time, he shows how the constitutional devices found in the United States—such as federalism, judicial review, and the separation of powers—can be adapted to inculcate a kind of moral virtue by teaching citizens to exercise liberty with moral responsibility and to govern themselves. The result is a constitutional theory that weaves ancient and modern principles into an original and coherent whole.*

Most U.S. citizens and legal scholars think of constitutional government as a system of legal and institutional restraints on power that protect private rights. To them, *constitutionalism* consists chiefly in knowing the number and kinds of rights that deserve constitutional protection or in examining the institutional mechanisms that limit and balance the powers of government. This commonly accepted view, however, is not the only conception of the term.

A different view is presented by Alexis de Tocqueville in *Democracy in America* (1945). As I shall argue, Tocqueville conceives of constitutionalism as more than a device for limiting power and protecting rights. Appealing to an older tradition, he argues that laws and institutions must be examined in light of the regime or whole social order underlying them. Such an analysis reveals that the modern democratic revolution has created a new kind of despotic control known as "popular sovereignty," which threatens to destroy the intellectual and moral character of citizens and make them unfit for liberty. Accordingly, constitutional government must go beyond the mere concern with constraining power through institutional mechanisms and take a deliberate interest in the moral education of citizens. This insight, I hope to show, leads Tocqueville to an interpretation of the U.S. Constitution that combines ancient and modern principles in an original theory.

Definitions of Constitutionalism

In the voluminous literature on constitutional government, a great deal of effort is devoted to determining what is meant by *constitutionalism.* The ambiguity of the term is such that most scholars acknowledge a variety of meanings, including some that seem to be mutually contradictory. For the purpose of under-

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standing Tocqueville's theory, we shall examine three of the most important definitions and then indicate how Tocqueville's conception may be distinguished from them.

The most general definition can be found in the works of such eminent legal scholars as McIlwain and Corwin. They define *constitutionalism*, in the broadest sense, as "the legal limitation on government . . . [whose] antithesis is arbitrary rule" (McIlwain 1947, 21) or "the belief in a law superior to the will of human governors" (Corwin [1928], 1971, 5). The emphasis here is on the limitation of power by law, specifically by a law that stands above the will of the rulers or the ordinary statute law. Such a fundamental law might be a written code or document; but more commonly it is an unwritten "higher law" that could be discovered, articulated, and enforced by a variety of authorities. Hence, both McIlwain and Corwin include a large number of higher-law theories in this category—classical natural right, Roman law, Christian natural law, English common law, modern natural rights, and the U.S. Constitution. So sweeping is this notion that Corwin claims to unfold an unbroken tradition of such theories from Aristotle and Cicero to John of Salisbury to Sir Edward Coke to Locke and Madison (Corwin [1928], 1971, 9-89; see also Wormuth 1949). This definition is obviously the most inclusive; for it treats every type of higher law as a kind of constitutionalism and views the differences among types of higher law as less significant than their status as norms or rules above the human will.

Though useful as a beginning point, this definition does not do justice to the variety of phenomena claiming the title *constitutionalism*. Hence, many scholars distinguish between *ancient* and *modern* constitutionalism, acknowledging thereby that the ancient Greeks and Romans meant by *constitutionalism* something quite different from what we think of today. McIlwain, for example, points out that the word often translated as "constitution" in the writings of Plato and Aristotle is *politeia*, a term referring to the entire social order of a city or nation as well as the form of government, "its whole economic and social texture as well as matters governmental in our narrow modern sense" (McIlwain 1947, 24; Wheare 1966, 1). In a recent article that attempts to resurrect the ancient meaning of *constitution*, Maddox refers to Cicero's use of *constitutio*—the Latin noun from which the English *constitution* is derived. It means "the 'shape,' 'composition,' or 'establishment' of a people in their political association" (Maddox 1982, 807). In both cases, the words emphasize the ordering of the whole association, the way its various parts are organized into an identifiable form or structure.

One might add to these observations that the classical philosophers and historians trace the form of a political association to the relation between ruler and ruled, specifically to the way the ruling group or dominant class imposes its ideas of what is just and honorable on the ruled and molds the characters and tastes of citizens. Perhaps the best translation of *politeia*, therefore, is not "constitution" but "polity" or "regime": the whole political and social order as it is shaped by the ruling group and its vision of the good life. Using the notion of a *regime*, classical political science seeks to identify the common patterns and to classify them according to the number and types of people who make up the ruling body—a *monarchy* is a regime ruled by one preeminent individual or family; an *oligarchy* is rule by the wealthy few; a *democracy* is rule by the common people, and so on.

The purpose of such a classification, of course, is not merely to describe structures of power but to guide the legislator in choosing the regime which is best or the best possible in the given circumstances.

Guided by a "higher-law" standard of natural right that prescribes *virtue* as man's natural end, ancient constitutionalism turns the classification of regimes into an exercise in moral choice, a problem of choosing the regime best suited to make men virtuous—to make them courageous, moderate, just, and prudent. For most of the classical philosophers and historians, this means choosing an aristocracy of wise and virtuous gentlemen in the ideal circumstances, a mixed or middle-class regime in less ideal circumstances, and a democracy tempered by rule of law and public spiritedness as a realistic third choice (Aristotle, *Politics* 3, 4, 7; Polybius, *Histories* 6). In speaking of ancient constitutionalism, then, one is referring to *regime analysis:* the classification of regimes and the prudential choice of which regime or combination of regimes is best suited to mold virtuous citizens.

Neither the general definition nor the ancient view, however, adequately captures the way *constitutionalism* is used today. The typical U.S. citizen, as well as the majority of contemporary legal scholars, use the terms *constitution* and *constitutional government* to mean something more precise than a higher law and more limited than the regime as a whole. They mean institutional restraints on power that protect private rights—such as the separation of powers, electoral accountability, or systems of checks and balances. According to McIlwain, this is the distinctively modern understanding of constitutionalism (McIlwain 1947, 115, 142). Another distinguished legal scholar, Sartori, calls this meaning of constitutionalism the *garantiste* meaning (emphasizing the guarantees for individual liberty) and insists that it is the only correct understanding of the word (Sartori 1962, 863; also, Friedrich 1946, 4, 21; Wheare 1951, 10-11; Wormuth 1949, 3). One should add that this definition expresses the view of James Madison and the other authors of the *Federalist Papers.* They viewed the U.S. Constitution as a system of checks and balances that limits the powers of the federal government in order to secure the rights of persons and property *(Federalist* nos. 9-10; Madison 1981, 36, 62). According to the modern conception, constitutionalism is grounded in the "higher-law" doctrine of natural or human rights and focuses not on the way regimes mold the characters of citizens but on the institutional mechanisms for limiting, dividing, and balancing the powers of government.

Ancient Constitutionalism and the Modern Phenomenon of Sovereignty

In turning to Tocqueville's analysis of the U.S. Constitution in *Democracy in America,* one senses that it does not fit neatly into any of the previously mentioned definitions of constitutionalism. At first glance, the conceptual framework of the whole of *Democracy in America* seems to follow the lines of ancient constitutionalism. It is a comparison and evaluation of two regimes: democracy and aristocracy (or its European variant, feudalism). Yet, as one scholar has noted, the terms Tocqueville employs to describe the regimes are sometimes confusing because they reflect both "political" and "social" categories (Richter 1970, 90; Richter 1969, 156-59). Thus, Tocqueville defines *democracy* as the rule of the people or popular sovereignty; but he traces the ultimate foundation of democracy to "equality of social condition," which he understands in socioeconomic terms as the absence of hereditary classes or fixed social hierarchies (vol. 1, Chaps. 3, 4). This leads some commentators to the conclusion that Tocqueville is one of the founders of modern sociology (Aron 1968, 237). A more accurate view, however, is that Tocqueville uses some of the language and categories of modern sociology but does so in the service of an older conception of political science. His emphasis on the role of social classes and their characteristic patterns of behavior is actually an emphasis on the *regime* in the classical sense (Zetterbaum 1967, 52). For the central theme of the book is that all aspects of life in democratic America are shaped by the ruling group, the sovereign people, and by belief in equality, which they establish as just and authoritative. Our first impression of Tocqueville, then, is generally correct: his enterprise is not sociology but the regime analysis of classical political science.

Yet, one cannot deny that Tocqueville continually expresses himself in language that gives primacy to social and cultural phenomena over those that are narrowly political. This tendency is particularly evident in his statements about the organization of *Democracy in America* and his method of analysis. Tocqueville states that his work is divided into two parts: the subject of the first part is the *laws* of the United States; the subject of the second is the *customs* (1:15). A glance at the table of contents of Part 1 indicates that by the *laws* he means more than the legislative acts of the U.S. government; he means the entire constitutional system—the federal and state constitutions, the formal and informal institutions of government, and the conceptions of justice underlying the laws and institutions. Although Tocqueville uses the term *laws* in a comprehensive sense to mean the constitutional system and devotes the first part (18 chapters) to explaining its operation, he clearly states that such matters are only one part of the whole phenomenon of democracy in the United States. He asserts that three causes or factors must be considered in order to understand the democratic phenomenon in America: the accidental circumstances (geography, climate, and historical contingencies), the laws (constitutions and institutions), and customs (mores or habits, sentiments, and beliefs). Of these three causes, the accidental circumstances are the least important, the laws are of secondary importance, and the customs are most important (vol. 1, Chap. 17). The implication is that laws are subordinate to customs, that political phenomena are shaped primarily by social and cultural phenomena. One might conclude, then, that Tocqueville employs the regime analysis of classical political science but modifies it by making social and cultural factors more important than narrowly political ones in the formation of regimes (Ceaser 1985, 656).

Despite the modern terminology, this approach is not in itself a departure from classical regime analysis because the classical conception of the regime rests on a very broad notion of the political—one that includes the role of social classes and their characteristic customs, mores, culture, or ethos (1:310). Tocqueville departs from the classical approach not in giving primacy to customs but in separating laws from customs. By creating a distinction between the two phenomena, he does something classical political scientists would never do, namely, separate the legal or constitutional system from the regime or whole social order. As a result, Tocqueville makes statements that would sound strange to classical ears: "The political Constitution of the United States appears to me to be one of the forms of government that a democratic people could adopt; but I do not regard the American Constitution as the best, or as the only one, that a democratic people may establish" (1:246). What is unusual about this statement is that Tocqueville equates the constitution with the form of government and distinguishes it from the regime; he then asserts that a democratic regime or democratic people could adopt any one of several forms of government and still remain a democracy. It could create a "democratic republic," as he says the American people have done, or it could create a monarchical constitution, as he prefers for France (Tocqueville 1970, 200; Tocqueville 1985, 113–14).

Tocqueville's distinction between law and custom, between constitution and regime, is a significant departure from classical political science because it separates the political realm from the larger social ethos and relegates the political to a matter of secondary importance. Instead of the form of government shaping the social ethos, as the classical philosophers maintain, the social ethos shapes (although it does not completely determine) the form of government. If we search for the reason behind this departure from ancient constitutionalism, it may be found in the modern notion of *sovereignty.* What most impresses Tocqueville about modern democracy is that it resembles the regimes of the past in certain respects but ultimately is based on a new and different kind of authority: it is not merely a regime but a condition of sovereignty. The difference between a regime and a condition of sovereignty, as we shall see, lies in the degree and manner of control exercised by the ruling body.

Tocqueville explains this difference in Chapter four of *Democracy in America,* entitled, "The Principle of the Sovereignty of the People in America." There he asserts that the principle of popular sovereignty actually exists at the bottom of all human institutions, including despotic governments; for the support or acquiescence of the people is required of all established institutions. In the past, however, the sovereignty of the people was concealed or was acknowledged only in empty phrases such as the *will of the nation* or the *commonweal.* By contrast, Tocqueville claims, in modern democracies like the United States "the principle of the sovereignty of the people is neither barren nor concealed, as it is in other nations." This changes the nature of political authority: "In some countries a power exists which, though it is in a degree foreign to the social body, directs it, and forces it to pursue a certain track. . . . But nothing of the kind is to be seen in the United States; there society governs itself for itself. All power centers in its bosom, and scarcely an individual is to be met with who would venture to conceive, or, still less, to express the idea of seeking it elsewhere" (1:59). In traditional forms of political authority, the ruling group (such as an aristocracy or a clergy) stands outside and above the people, shaping and forming the people to its ends; the state molds society, creating a regime. In modern democratic societies, by contrast, the ruling group does not shape the people, the state does not mold society, but "society governs itself for itself." This kind of authority differs from earlier kinds because it is not based on a distinction between a ruling part and a ruled part; the two parts are identical. No need exists for one part to mold and shape the other (as in a regime) because there is no challenge or resistance. The state does not mold society but is absorbed by society and becomes subordinate to society. This is a condition of sovereignty because there exists an unchallenged, indisputable, and all-absorbing power that dominates without ruling because there is no conceivable alternative to its power.

Obviously, it is from this notion of sovereignty that Tocqueville draws his dire warnings about "the unlimited power of the majority in the United States" (vol. 1, Chap. 15). In expressing his fears about the tyrannical power of the majority, Tocqueville goes well beyond the criticisms of democracy leveled by the classical philosophers or by democratic theoreticians in modern times, like James Madison. Their warnings were focused on the political dangers of majority rule, such as the dispossession of the rich, the ostracism of the great, or the suppression of minority rights (Aristotle, *Politics* 3; *Federalist* no. 10). For Tocqueville (and, following him, J. S. Mill), the tyranny of the majority is not only a political phenomenon but also an intellectual and moral phenomenon—an enslavement of the mind and a weakening of the will arising from the inability to conceive of any sort of authority except that of the people, or any standard of legitimacy except that of majority opinion. Even the harshest critic of democracy in the ancient world, Plato in the eighth book of *The Republic*, does not single out democracy for being more intellectually and morally despotic than other regimes; he contends that all regimes are restrictive and exclusive in the way they impose their notions of justice and honor on society. In fact, he sees more diversity and openness in democracy (which even tolerates the philosopher, up to a point) than in other regimes. What Tocqueville describes as the sovereignty of the people, the empire of the people, the irresistable pressure of popular taste and culture, is something that is not fully captured by the classical notion of the *regime*. With a sense of profound awe before a newly discovered mystery, Tocqueville says, "When I compare the Greek and Roman republics with these American states . . . When I remember all the attempts that are made to judge the modern republics by the aid of those of antiquity, and to infer what will happen in our time from what took place two thousand years ago, I am tempted to burn my books in order to apply none but novel ideas to so novel a condition of society" (1:327). With a sense of forboding, he also says, "I think, then, that the species of oppression by which democratic nations are menaced is unlike anything that ever existed before in the world" (2:336). The novel condition that Tocqueville witnesses and seeks to understand is the modern phenomenon of democratic sovereignty.

If we search for Tocqueville's explanation of the origin and cause of this new phenomenon, we cannot find it in his discussion of popular sovereignty or majority tyranny in volume 1 of *Democracy in America*; for the first volume focuses on the laws, the legal or constitutional system as it is shaped by the principle of popular sovereignty. To understand not only the effects but also the cause of popular sovereignty, we must turn to volume 2, where the customs, or mores, of the people are analyzed; for the phenomenon of sovereignty is caused by a way of thinking, a way of conceiving of authority. From his discussion of democratic mores, we learn that attitudes toward authority in the United States are shaped by a mode of thinking and feeling that Tocqueville calls *individualism*. He argues that individualism is largely a product of the modern Enlightenment (as well as the Protestant Reformation), which taught modern men to question all authorities and to look for truth only in their own independent thinking—a mental habit, in other words, of skepticism and individual self-reliance (2:4-5). The effect of this mental habit is a startling and profound paradox in the way that modern democratic men conceive of authority; for it does not liberate the human mind in the way that the Enlightenment thinkers originally hoped and expected. According to Tocqueville, when most men are called upon to free themselves from higher authorities, they become frightened and confused and feel the insignificance of their own thoughts; instead of thinking for themselves, they turn to public opinion or the voice of the majority for guidance (2:11). The result is a paradoxical combination of individualism and conformity—an insistence on the equal right to think for one's self combined with a silent surrender of the mind and bending of the will to a new authority, the sovereign people.

To a certain extent, Tocqueville admits, this is only natural: "men will never cease to entertain some opinions on trust" without questioning or examining them;

radical and perpetual doubt about first principles leads either to madness or chaos; dogmatic beliefs of some kind are necessary for life. But the new conformity underlying democratic individualism is actually more slavish and dogmatic than, for example, the religious authority of the past precisely because it appears on the surface to be intellectual freedom and is rarely acknowledged as a species of intellectual dogmatism. Its disguises make it difficult to recognize, for it imposes itself not by active assertion but by passivity, complacency, and casual acceptance—by the relaxation of the mind and will that occurs when the demands of higher authority are questioned. Tocqueville suggests that even those "minorities" who might be suspected of disloyalty to democrary, such as the wealthy, the clergy, and the literary or intellectual elite, have difficulty resisting the pressures of popular taste and culture (1:54, 312-20; 2:36-64). For the principle of popular sovereignty, or will of the majority or voice of "the public," triumphs without argument "through a sort of enormous pressure of the mind of all upon the individual intelligence" (2:9-12).

In short, because modern democracy is founded not merely on the rule of the people but on the popular mind as it is filtered through the modern Enlightenment, the whole conception of authority is revolutionized. The sovereignty of the people comes into being, characterized by freethinking individualism and an uncontested surrender to the will of the people. The result, according to Tocqueville, is a despotism of the mind and weakening of the character such as the world has never seen before, raising the specter of a new kind of democratic tyranny.

Following this analysis, we can say that Tocqueville's *constitutionalism* is a combination of old ideas and new categories adapted to fit the phenomenon of modern democracy (Richter 1969, 159). When Tocqueville observes the political and social developments of his day, he thinks broadly in terms of regimes, primarily the contrast between aristocracy and democracy or between "the old regime" (to use Tocqueville's phrase for European feudalism) and the new democratic order. But, the regime analysis of ancient constitutionalism cannot fully grasp the new democratic order because the concept of the *regime* does not adequately describe the kind of domination and potential for despotism of modern *sovereignty.* Moreover, the classical concept of the regime does not account for the way that modern societies distinguish between the sovereign and the government, the customs and the laws, the society and the state, or the sovereign people and the *constitution* (in the narrow and limited sense in which the term henceforth is used). As Tocqueville says in the introduction to his work, "a new science of politics is needed for a new world," and his adaption of classical regime analysis to fit the phenomena of democratic sovereignty and the limited constitution is the centerpiece of the new science.

Adapting the Regime of Virtue to Democratic Sovereignty

In presenting Tocqueville's modification of ancient constitutionalism, we have concentrated almost exclusively on its descriptive features, on its use as an analytical tool to describe the nature of power and authority. Yet, ancient constitutionalism is also, indeed it is primarily, a normative theory. It is essentially an exercise in political prudence. The classification of regimes and the ideal of a regime of virtue are intended to be general guides for the legislator, whose task is to choose the best possible political order in the given circumstances, the regime most conducive to the formation of virtuous citizens under the existing conditions.

In *Democracy in America,* Tocqueville

adopts the normative orientation of ancient constitutionalism. He accepts the traditional view that politics should be directed to the formation of virtuous characters through moral and civic education. But he recognizes that new techniques or a modification of old ones are necessary to achieve this goal, for it is no longer possible to create a regime of virtue by educating a ruling class and having it impose its morals from the top down, so to speak, through laws and institutions. Modern democracy prohibits such an "authoritarian" imposition of morals; it prohibits a regime of virtue in the strict sense because it rejects in principle the notion of higher authorities or the rule of moral superiors. In place of a regime imposed from the top, modern democracy creates a sovereign, the people, who in turn create a constitution, their state and form of government, whose purpose is to serve their needs and protect their rights. In these circumstances, the inculcation of virtue must follow a different strategy, one that is indirect and even hidden from view. Most importantly, as we shall see, it must speak the language of democracy, which means it must appear not as a form of virtue or moral superiority but as a form of liberty.

In one of his most explicit statements on first principles in *Democracy in America,* Tocqueville proclaims the highest ethical principle to be virtue and asserts that virtue should be the standard and guide for politics: "After the general idea of virtue, I know of no higher principle than that of right; or rather these two ideas are united in one. The idea of right is simply that of virtue introduced into the political world. It was the idea of right that enabled men to be independent without arrogance and to obey without servility. . . . There are no great men without virtue; and there are no great nations—it may almost be added, there would be no society—without respect for right" (1:254). Without actually defining *virtue* in this passage, Tocqueville asserts that it is the highest ethical principle because it is the source of human greatness for individuals and nations. Virtue is the foundation of individual character, indeed, it is the perfection of human character. In the political world, it is the foundation of political right or notions of legitimate authority that enable men to obey authority freely, through voluntary self-restraint, without being tyrannical or servile. The type of virtue that Tocqueville has in mind here (and that he praises repeatedly in all his writings) appears to be a synthesis of civic and heroic virtue. Following Montesquieu and Rousseau, he defines *civic virtue* as republican virtue: it is patriotism or love of country over love of self, a sentiment that Tocqueville often associates with self-assertion on the one hand and mastery of the selfish passions on the other (1:97; Tocqueville 1955, 17; Schleifer 1980, 242-43). By *heroic virtue,* Tocqueville usually means great feats of courage and daring in war as well as chivalry and great acts of principled statesmanship—ideals he instinctively absorbed from his aristocratic heritage, from his reading of Plutarch, and perhaps from contemporary romantic notions of the Napoleonic hero (2:242-55; Tocqueville 1955, 118-20; Tocqueville 1985, 124-25; Boesche 1981). Whatever his sources of inspiration, Tocqueville holds civic and heroic virtue to be the highest ethical ideal because it produces noble characters, characters whose proud self-assertion and voluntary self-restraint make them true citizens and statesmen.

While appealing to the ancient ideal of virtue, Tocqueville recognizes the need to adapt it to the conditions of modern democracy. For democracy produces many forces that tend to undermine the assertive pride and self-restraint of a virtuous character. We have already seen how the pressure of popular sovereignty wears down the intellectual independence and strength of will of those who claim moral superiority to the majority. In addition, Tocqueville argues that democracy unleashes certain feelings or passions—an ethos of individualism and materialism—that endanger virtue. He sees *individualism* not only as a mental habit of questioning traditional authorities but also as a passion that results from the weakening of traditional social bonds; it is a peculiar kind of selfishness that combines egoism with a tendency to isolate one's self from others (2:102). *Materialism* is the love of physical well-being, especially the love of comforts and conveniences that make life softer and easier, a passion most characteristic of the middle classes (2:137). In the democratic age, these two passions are usually joined in an ethic of self-improvement, which drives individuals in a competitive and energetic quest for wealth and success (2:165). Tocqueville's view is that such passions are especially harmful to the moral character of citizens. For they tend to encourage withdrawal from public life by channeling all energy into business and commercial activities; ultimately they may destroy every form of individual initiative and restraint, turning the masses into passive sheep (2:149, 335-37).

Throughout his analysis of individualism and materialism, Tocqueville states explicitly that such tendencies are inimical to virtue: "Selfishness blights the germ of all virtue; individualism, at first, only saps the virtues of public life; but in the long run it attacks and destroys all others. . . . Selfishness is a vice as old as the world, which does not belong to one form of society more than to another; individualism is of democratic origin" (2:104). In a more sweeping statement about the effects of individualism and the love of material well-being, Tocqueville says that "the age of implicit self-sacrifice and instinctive virtues is already flitting far away from us" (2:132). Such statements make abundantly clear that the demise of virtue is Tocqueville's primary concern in judging the moral character of modern democracy. In one of his notebooks, he states succinctly, "Egoism has replaced virtue"—that is the moral problem of democracy (Schleifer 1980, 241).

While making these judgments, Tocqueville recognizes that traditional civic and heroic virtues can never be resurrected in their pure and original forms. The task of the legislator, therefore, is to find substitutes for virtue compatible with democratic habits and beliefs. In surveying the U.S. scene, Tocqueville sees the greatest hope in the American understanding and practice of liberty. In analyzing this phenomenon, he presents an original and insightful interpretation that shows that American liberty assumes two distinct forms, political liberty and individual rights, and that both kinds of liberty can serve as democratic substitutes for virtue.

Tocqueville's account of political liberty appears at the beginning of *Democracy in America* and is boldly proclaimed as "the germ of all that is to follow and the key to almost the whole work" (1:29). He explains how the tradition of political liberty began in America with the settling of New England by the Puritans. In Tocqueville's eyes, the Puritans were remarkable because they combined strict religious beliefs—which prohibited everything from idleness and drunkenness to kissing in public and long hair—and political freedom. Their laws reflected the severity of Puritan morality; yet, "it must not be forgotten," Tocqueville says, "that these fantastic and oppressive laws were not imposed by authority, but that they were freely voted on by all persons interested in them" (1:40-41). By living according to strict laws that were voluntarily imposed, the Puritans were able to unite two things that other societies have found incompatible: the spirit of religion and the spirit of liberty. Through this unusual synthesis, the Puritans created the ideal of political liberty that permeates the whole of U.S.

society. In New England, it was embodied in townships and town meetings, where citizens participated directly in law making and administered local affairs. Spreading in all directions from its original source, the ideal of political liberty has "interpenetrated the whole American confederation and now constitutes the social theory of the United States" because it is the purest or most direct form of democratic self-government (1:32).

In tracing the origins of political liberty to the Puritan tradition of self-government, Tocqueville also uncovers its moral foundations. It rests, he says, on "a fine definition of liberty," a definition best expressed in a speech by Jonathan Winthrop, the early governor of Massachusetts. The gist of Winthrop's speech (as quoted by Tocqueville) is a distinction between two types of liberty. The first is *natural liberty*, which is the freedom to do what one wants and is no different from animal freedom. The second is *civil* or *moral liberty*, which is the freedom to do only what is good, just, and honest; it is a type of freedom that arises out of a covenant between humanity and God and must be exercised "in subjection to authority." According to Tocqueville, moral liberty is higher and more noble than natural liberty because it involves self-restraint, or mastery of the passions, and is bounded by moral and intellectual limits that prevent it from degenerating into license or chaos. Such liberty requires religion or some sense of sacred awe "before truths which it accepts without discussion" in order to provide a sense of limit and restraint (1:44-45). Such liberty, in turn, is the basis of democratic self-government; for it gives people the discipline and self-restraint to live by self-imposed laws, as well as the respect for authority and sense of civic duty to participate in public life. Thus, the moral liberty of the stern and pious Puritans is the foundation of political liberty or self-government in the United States.

The significance of this account is in providing a conception of liberty that closely resembles the classical notion of civic or republican virtue. The description of moral liberty provided by Jonathan Winthrop and warmly embraced by Tocqueville is closely akin to virtue because it is based on a hierarchy of the soul in which the higher or divine part rules over the lower or animal part in conformity with a higher law. It is also like virtue in that it requires people to subordinate their private interest to the public good. Moral liberty is different from classical republican virtue, however, because of its Puritan foundations. It arises from the Protestant idea of Christian liberty, which requires people to obey divine law from a free and willing choice rather than from sentiment, habituation, or the coercion of external authority. Moral liberty is self-willed obligation or obedience to a self-imposed law. Thus, it is properly classified as a species of freedom rather than of virtue, although the two are very similar.

The great question for Tocqueville is what becomes of moral liberty and the democratic self-government it sustains once the Puritan tradition grows weak and dies out. When this occurs, moral liberty is no longer a free and voluntary obedience to divine law but obedience to merely human law or perhaps to no law. Moral liberty then loses the inner discipline which distinguishes it from natural liberty, and democracy is left without restraints. In a sense, one could read volume 1 of *Democracy in America* as a moral melodrama, a kind of *Paradise Lost* in which democracy falls from the grace of the original Puritans to the despotic excesses of the tyrannical majority. As the story unfolds, about three-fourths of the way through volume 1, Tocqueville reveals that the old religious faith that once nurtured self-government in the United States lost its power and must be replaced by a new (but far less noble) moral principle: "Do you not see that religious belief is shaken and the divine notion of right is declining, that morality is debased and the notion of moral right is therefore fading away? Argument is substituted for faith, and calculation for the impulses of sentiment. If, in the midst of this general disruption, you do not succeed in connecting the notion of right with that of private interest, which is the only immutable point in the human heart, what means will you have of governing the world except by fear?" (1:255). Striking a note of pessimism, Tocqueville reveals the fate of the Puritan ideal. The old faith has lost its power because the advancing tide of modern skepticism has weakened the notion of divine right and replaced it with enlightened self-interest. In this age of skepticism, the only secure basis for political right is self-interest or private interest. A doctrine that builds upon self-interest and is capable of turning it into a moral principle—a principle of right that makes obedience to authority a matter of voluntary compliance rather than of coercion and fear—must be found.

At this point, Tocqueville turns, in a spirit of prudent compromise, to the doctrine of individual rights: "I am persuaded that the only means which we possess at the present time of inculcating the idea of right and of rendering it, as it were, palpable to the senses, is to endow all with the peaceful exercise of certain rights." The most important ones are property rights. For an individual may have "no notion of the property of others; but as he gradually learns the value of things and begins to perceive that he may in his turn be despoiled, he becomes more circumspect, and he ends by respecting those rights in others which he wishes to have respected in himself" (1:254). Tocqueville's claim is that in an age when divine right and traditional virtues are declining, the only effective moral principle is the doctrine of individual rights, especially property rights. This defense of rights, of course, is purely prudential. For rights find powerful support in human selfishness and thus can be seen as nurturing moral vice or social instability. Yet, when properly understood as things that must be respected in others in order to be enjoyed by oneself, they turn self-interest into a principle of right. Through self-interest, rights lead people to self-restraint, respect for fellow citizens, and respect for authority. Rights thereby become another substitute for virtue, although a less satisfactory substitute than political or moral liberty.

The difficulty that remains for Tocqueville is to find a common moral basis for these distinct notions of liberty; for the two are not entirely compatible. Political liberty is a moral or theological conception derived from the Puritan tradition. It requires people to govern themselves in accordance with divine law; it frees them from the enslavement to their base and selfish passions by making them accountable to a higher law and teaches citizens to subordinate their private selves to the public good. While Tocqueville calls this a "fine," or noble, definition of liberty, he recognizes that its religious foundation will probably not survive in the enlightened and secular age of democracy (although he feels every effort must be made to perpetuate it as long as possible in the customs of Christian America [Kessler 1977]). By contrast, the second notion of liberty, individual rights, is perfectly at home in the enlightened and secular age but lacks a notion of moral restraint or accountability to a higher law; it encourages private selfishness and promotes restraint and respect for authority only through enlightened self-interest. Its foundation is more powerful and lasting but less noble.

To resolve this dilemma, Tocqueville looks to a phenomenon that is already present in American customs and political life, although imperfectly perceived and understood. That phenomenon is a sense of moral responsibility—the belief that human freedom involves moral obligations because human beings are masters of their fate and must have the willingness and capacity to govern themselves. This notion of moral responsibility, Tocqueville suggests, is a secular counterpart or secular legacy of the old moral liberty of the Puritans (1:67, 94-96). Like the Puritan ideal, it requires self-government in the political and personal sense. It requires people to determine their destiny by participating in law making and freely submitting to their self-made laws; and it requires individuals to take control of their lives and be accountable for the consequences of their actions (Lively 1962, 221-28). However, it locates the ground of responsibility not in obedience to divine law but in human pride—in the pride and dignity of free people who seek to improve their condition in this world by mastering their fate, controling their destiny, and developing the courage, intelligence, and discipline to govern themselves. Moral responsibility in this sense is a wholly secular notion that combines civic duty and self-interest, thereby bridging the gap between the two notions of liberty.

According to Tocqueville, the most admirable features of democratic liberty can be traced to the notion of moral responsibility. It inspires the cooperative endeavors of local government and civic associations, based on the principle of "self-interest rightly understood." Such actions are motivated by the desire of citizens to improve their condition and benefit themselves, while requiring them to develop some of the virtues of self-government (2:127-32). Moral responsibility also inspires the initiative, enterprise, and daring that drives commercial and technological progress; it engenders the pioneering spirit in people who boldly seize the future and make it work for themselves (1:260-61). The same moral sense can be found in the patriotism and respect for law that is peculiar to modern democratic nations and is especially pronounced in the United States. Such patriotism is different from the sentimental attachment to birthplace and almost religious reverence for the sovereign found in the ancient monarchies of Europe. Democratic patriotism is neither sentimental nor instinctive but rational and reflective; it is the feeling of a citizen who believes the laws and prosperity of the nation are partly the citizens' own creations and are beneficial to them; it is the possessive pride felt by a citizen who consciously says, "This government is mine and works for me" (1:251). This "rational sentiment" also creates respect for the rule of law among democratic citizens because they recognize that laws are voluntarily self-imposed and may be changed if they are harmful (1:258). According to Tocqueville, all of these activities and attitudes arise from the belief that human beings are responsible for their fate and must have the willingness and capability to govern themselves.

In a grand summary statement, Tocqueville asserts that "the greatest advantages of democracy" may be traced to the belief in moral responsibility that underlies democratic liberty. It imparts to "the humblest individual . . . a certain degree of self-respect" and a desire to be "better informed and more active" and to improve his or her condition. It creates "an all-pervading and restless activity, a superabundant force and energy" in the whole society (1:260-61). From such statements, we may infer that Tocqueville praises the notion of moral responsibility because of its effect on the characters of free people: it produces the proud self-assertion and voluntary self-restraint of virtuous citizens. Moral responsibility, in short, raises democratic liberty to the rank of virtue.

The American Constitution Correctly Understood

Using these insights about the problems of democratic sovereignty and the formation of virtuous characters, Tocqueville develops a distinctive interpretation of the U.S. Constitution. He recognizes that the modern democratic revolution has fundamentally altered the nature of political authority. It has created a distinction between the sovereign and the government or, more precisely, between the sovereign people and their political constitution, which drastically reduces the scope and power of the constitution. In modern democratic societies, the dominant force is the sovereignty of the people or the omnipotent majority; it acts like a *regime*, ordering and shaping the whole of society, while the *constitution* (in the narrower sense) is a limited and artificial device. A modern constitutional system, such as the one found in the United States, is a set of legal and institutional mechanisms designed to serve the needs and protect the rights of the people. As such, it is a kind of epiphenomenon that is largely shaped by the underlying forces of democratic sovereignty and the historical contingencies from which it arises.

In these circumstances, Tocqueville approaches the U.S. Constitution with two objects in mind: to show the debilitating effects of majority tyranny and to show the methods for promoting morally responsible liberty. As a student and admirer of *The Federalist*, he recognizes that the framers of the U.S. Constitution shared these concerns when they created a Republican constitution based on the principle of limited majority rule (1:143, 158-59). Since the founding, however, it has become evident that the constitutional mechanisms created by the U.S. framers—the whole "Madisonian" system of checks and balances—are insufficient to stop, and may even encourage, the pressure of democratic sovereignty and majority tyranny. Indeed, Tocqueville warns that the U.S. constitutional system is in danger of being completely absorbed by the social forces of the majority, threatening not only the individual rights it is designed to protect but also the intellectual and moral character of citizens (1:60).

Writing in the 1830s (one generation since the founding), Tocqueville observes that the U.S. constitutional system suffers from a decline in the moral quality of leaders and citizens, reflected in a general weakening of character and a relaxation of moral restraints (1:143). He also notes many specific abuses of power. One is the tendency of the legislative branch to dominate all others, an expression of majority tyranny that the framers sought to, but failed to, overcome: "The Americans have not been able to counteract the tendency which legislative assemblies have to get possession of the government" (1:126). Another problem is the extreme weakness of the executive. Tocqueville calls it "an inferior and dependent power" compared to Congress and describes the process of electing and reelecting presidents as "a national crisis" or "a sort of revolution," which stops the wheels of government and turns the executive into "an easy tool in the hands of the majority" (1:142-43). All of these problems can be seen in the behavior of President Andrew Jackson; Tocqueville views him as a populist demagogue, "a slave of the majority," who attacks the powers of the federal government and thereby contributes to both the despotism of the majority and the fragmentation of the union (1:431-32). This points to a third problem, the weakness of the federal system itself. While it possesses the virtue of combining the power and glory of a great nation with the liberty and well-being of small communities, it has an inherent weakness: the government of the union is an artificial creation, "a legal fiction," and therefore commands less authority and affection than state and local governments; the dissolution of the union, Tocqueville warns, is more than a remote possibility (1:172, 432). Thus, the existing constitutional system in the United States is deficient because it fails to restrain the majority and to contain the centrifugal forces of competing interests. It suffers, one might say, from both the tyranny and the anarchy of the majority.

The reason for these deficiencies, Tocqueville suggests, is that the framers provided legal and mechanical remedies, such as divisions of power or countervailing forces, for what is essentially a moral problem: the tyranny of the majority is ultimately due to a loss of virtue. In its fullest sense, it is a weakening of intellectual and moral character by the whole ethos of modern democracy; it creates despotic tendencies because the people lose the willingness to take responsibility for their lives and the capacity to govern themselves. One way that Tocqueville responds to this problem in *Democracy in America* is by emphasizing the Puritan origins of the U.S. constitutional system (at the beginning of volume 1) and by continuously stressing the primacy of mores to laws. His second solution, which focuses on the laws themselves, is to combine the wisdom of the Puritans with that of the framers in a vision of the constitutional system that stresses the moral education of citizens rather than the mere division of power through institutional mechanisms or checks and balances.

In a passage reflecting both the limitations and the dignity of laws in the democratic age, Tocqueville says, "Laws cannot rekindle an extinguished faith, but men may be interested by the laws in the fate of their country. It depends upon the laws to awaken and direct the vague impulses of patriotism, which never abandons the human heart; and if it be connected with the thoughts, the passions, and the daily habits of life, it may be consolidated into a durable and rational sentiment" (1:97). The function of laws, Tocqueville suggests, is to fill the void left by the decay of religion with the inculcation of patriotism. With its echoes of modern nationalism or even totalitarianism, this statement might sound "illiberal" or dangerous. In the context of *Democracy in America,* however, the rational sentiment of patriotism to which Tocqueville alludes is identical to the morally responsible liberty, or genuine self-government, that Tocqueville defends as the modern form of civic virtue; for it requires citizens to take an "interest in the fate of their country," that is, to possess the assertive pride that inspires people to control their destiny and master their fate. At the same time, it requires them to obey their self-imposed laws and to exercise self-restraint. Next to religion, Tocqueville argues, such patriotism is the only force that can voluntarily unite people's wills without creating new forms of despotism (1:97).

In showing how the laws can serve as instruments of moral education, Tocqueville reinterprets and in some cases modifies the constitutional devices created by the framers, such as federalism, judicial review, and the separation of powers. The thrust of his proposals is at first glance contradictory: he recommends greater decentralization of the American federal system as well as greater concentration of power on the national level. The consistency of these proposals, as we shall see, lies in Tocqueville's insight that morally responsible liberty requires the laws to empower the majority by direct involvement at the local level while restraining the majority by independent authority on the national level.

Tocqueville's praise of local government and a decentralized federal system is undoubtedly the most familiar feature of his constitutional analysis. Yet, as Schleifer points out, Tocqueville's support for decentralization is actually rather strange; for the exercise of local government would seem to provide new opportunities for majority tyranny and to support local tyranny rather than local liberty (1980, 212ff). Schleifer's inability to resolve this difficulty is due to insufficient clarity about Tocqueville's notions of majority tyranny and responsible liberty. Tocqueville argues that local government gives even the humblest citizen a chance to hold office or to participate in decision making; without this experience, he says, people become "indifferent to their fate" and "sacrifice their free will," that is, they lose the sense of being masters of their fate (1:96). Through direct participation, they realize that "men must walk in freedom and be responsible for their acts, or remain passive spectators . . . in schemes with which they are unacquainted" (1:94). Thus, decentralization may loosen external controls on local majorities, but it mitigates majority tyranny because the essence of such tyranny is not mere conformity but conformity through weakness of will—through passivity, complacency, mediocrity, and resentment of superiority in any form. By contrast, the sense that human beings can control their own destiny through political participation encourages assertive pride and energetic activism on the one hand and obedience to self-imposed laws and voluntary self-restraint on the other (1:98). Such conformity is not tyranny but morally responsible liberty, the liberty of people who are masters of their fate and who govern themselves through laws freely determined and freely obeyed.

Another puzzle in Tocqueville's praise of decentralization is his claim that local government contributes to patriotic unity rather than to political fragmentation. The solution lies in his understanding of democratic patriotism: it springs not from a sentimental attachment to one's birthplace but from a rational attachment to laws and government; it is the feeling of someone who consciously says, "This government is mine and works for me." Obviously, such rational patriotism arises most naturally from small communities where citizens have the keenest sense that government is their own and contributes to their prosperity. But the attachment to an atstract political entity rather than to a particular plot of land makes it possible for "every citizen of the United States [to] transfer, so to speak, his attachment to his little republic into the common store of American patriotism" (1:170). Thus, a decentralized federal system helps to mitigate the anarchy as well as the tyranny of the majority because the feeling of possessing or being a part of the government fosters a patriotic pride in the nation as whole.

This analysis of decentralization involves a subtle but important shift from the founders' view of federalism because "decentralization is equated with local government and opposed to state and even county government" (Winthrop 1976, 95). Indeed, Tocqueville downplays the states and denigrates the notion of states rights. He regards most of the state constitutions as defective—the terms of office are too short, the legislative bodies dominate the executive, "the despotism of the majority prevails"—and he views the states as the main source of fragmentation and disunity in the nation (1:160, 431). Tocqueville's intention, therefore, is to weaken the states while strengthening local government and the union as a whole.

Another institution that empowers local majorities by requiring direct participation is the trial by jury. Although Tocqueville clearly recognizes this institution as a form of popular sovereignty, he praises it for two reasons. It is a mechanism of a modern constitutional system that acts like a regime, directly imposing the norms of the ruling body on the ruled. The presiding judge is a moral educator, imparting the majesty of the law and legal notions of equity and proper procedure to ordinary citizens. In addition, the citizens themselves make decisions with fateful consequences for the accused, which provides a lesson in moral responsibility. In Tocqueville's words, "the jury teaches every man not to recoil before the responsibility of his own actions and impresses him with that manly confidence without which no political virtue can exist. It invests each citizen with a kind of magistracy; it makes them all feel the duties which they are bound to discharge towards society. . . . By obliging men to turn their attention to affairs other than their own, it rubs off that private selfishness which is the rust of society" (1:295). This passage is a clear expression of Tocqueville's view that political participation in making decisions ennobles the character of citizens by engendering "confidence" and requiring transcendence of "private selfishness"—an exercise in democratic liberty that shines like political virtue because it gives citizens the pride to take responsibility for their acts and the self-discipline to step outside the narrow circle of their personal lives.

While Tocqueville's praise of local government and jury duty are well known, his calls for greater centralization and concentration of power in the United States are often overlooked—lost, perhaps, in his warnings about the possibilities for new forms of "Caesarism" in volume 1 (p. 341) or the "soft despotism" of a centralized bureaucratic state in volume 2 (p. 335ff). Yet Tocqueville clearly favors greater centralization, of the proper kind, for the United States. His goal is to empower the majority at the local level while seeking greater independence from majority will on the national level by strengthening the federal judiciary and concentrating more power in the executive branch.

These recommendations involve a subtle reinterpretation of the doctrine of separated powers. Most importantly, Tocqueville does not see the separation of powers as a balancing of three roughly coequal branches; rather, he gives primacy to the judiciary. He praises the U.S. framers for creating a powerful federal judiciary, especially by granting judges the power to declare laws unconstitutional (judicial review). This power, Tocqueville points out, is a unique feature of U.S. constitutionalism, not to be found in the nations of Europe where judges only decide matters of fact or interpret the law when it is contested in a specific case. The limitations on judicial power in European nations result from their conceptions of the constitution as either an immutable entity beyond the power of judges to alter (the French view) or one that is changeable by the ordinary legislative powers (the English view). Compared to the French and English views, Tocqueville claims, "The political theories of America are more simple and rational. An American constitution is not supposed to be immutable, as in France; nor is it susceptible of modification by the ordinary [legislative] powers of society, as in England. It constitutes a detached whole, which, as it represents the will of the whole people, is no less binding on the legislator than on the private citizens, but which may be altered by the will of the people in predetermined cases according to established rules. In America, the Constitution may vary; but as long as it exists, it is the origin of all authority, and the sole vehicle of the predominating force" (1:105). The U.S. Constitution, in Tocqueville's telling phrase, is a "detached whole" —a fundamental law that stands outside and above the government and the people. Hence, there are two orders of law in the United States, the constitutional and the legislative. The constitutional law is the more fundamental of the two and functions like a "higher law": it cannot be modified by a legislative act and is the standard to which statute or positive law must conform (Pierson 1938, 603-4). Yet, the Constitution is not really a higher law in the traditional sense. Unlike divine or natural law it does not exist independently of the human will; it is created by the will of the people and may be altered or amended by the people according to preestablished rules. However, as long as it exists, it is absolutely binding on the people and their representatives; the duty of judges is to remind people of this obligation by striking down (or refusing to enforce) laws contrary to the Constitution (1:106). In the process, the people learn the most important lesson of moral responsibility: that they are free to create a fundamental law but must live within the boundaries of a self-created authority. Understood in this way, judicial review functions not only as a check on legislative despotism but as an instrument of moral education.

In addition to teaching citizens and leaders to live responsibly, the judiciary serves an important political purpose. This political purpose, however, is quite different from the contemporary phenomenon of judicial activism—the creation of law or policy by the courts through a flexible interpretation of the Constitution. Tocqueville states that a U.S. judge preserves his moral authority because he does not "attack the legislator openly and directly . . . [but] is brought into the political arena independently of his will" by deciding a case brought by a third party (1:107). He even warns explicitly against judicial activism by saying that "an essential quality of judicial power is never to volunteer its assistance to the oppressed, but always to be at the disposal of the humblest who solicit it" (2:343). The political role of the courts is very different from pronouncing abstract ideals or initiating social change. As Tocqueville says, although the activity of the Supreme Court is "essentially judicial, its prerogatives are almost solely political; its sole object is to enforce the execution of the laws of the Union." He contends that without the federal judges "the Constitution would be a dead letter: the executive appeals to them for assistance against encroachments of the legislative power; the legislature demands their protection against the assaults of the executive; they defend the Union from the disobedience of the states, the states from the exaggerated claims of the Union, the public interest against private interests, and the conservative spirit of stability against the fickleness of democracy. Their power is enormous, but it is the power of public opinion" (1:157). The political role assigned to the federal judiciary is the conservative one of preserving the unity and wholeness of the union against the jarring and clashing of competing powers that have been built into the American constitutional system.

In this statement one can discern a simple but powerful criticism of the entire Madisonian conception of checks and balances. When power is divided into many centers—when the three branches of government vie with each other, when the several states compete against each other and against the federal government, when diverse interest groups compete and conflict—what remains to hold the system together? Tocqueville's answer is "public opinion," but it is the opinion of the public as shaped by the judiciary in "enforcing" the Constitution, the humanly created system of higher law.

In order to be effective, Tocqueville adds, the justices of the Supreme Court must rise to the level of impartial statesmen, exercising a special kind of political prudence: "Not only must the Federal Judges be good citizens and men of information and integrity, which are indispensable to all magistrates, but they must be statesmen, wise to discern the signs of the times, not afraid to brave the obstacles that can be subdued, nor slow to turn away from a current when it threatens to sweep them off . . . [for] if the Supreme Court is ever composed of imprudent or bad men, the Union may be plunged into anarchy or civil war" (1:157). In the final analysis, then, it is not the separation of powers and its checks and balances that make the constitutional system work. It is a single branch, the federal judiciary, that binds the system together into a whole with the power of public opinion and moderates the clash of competing powers and interests. In a curious way, Tocqueville sees the judiciary as the ultimate "executive" of the laws: its primary task is the enforcement of law and its peculiar skill is to enforce the law through the exercise of moral authority—by prudently guiding public opinion. This vision of judicial power as an enforcement power (which resembles Tocqueville's description of the "justices of the peace" on the county level [1:77]) is more political than the view of the founders and less political than contemporary ideas of judicial activism or administrative law. For it gives the federal judiciary the political mission of preserving the union but does not challenge judges to initiate social change by expanding rights or to enforce laws through creative rule making. On the contrary, Tocqueville's appeal is to a kind of judicial statesmanship whose task is to teach all parties to live responsibly within the limits of their self-imposed law, the Constitution—a lesson in moral restraint.

Tocqueville makes a similar though less conclusive argument in recommending a stronger executive. His primary purpose is to expose the weakness of the U.S. president by comparing him to the constitutional king of France. The latter is a more powerful executive for two reasons. First, in addition to executing the laws, the king plays a dominant role in law making by initiating legislation, exercising an absolute veto, and appointing members in one chamber and dissolving the other, at will. Second, he holds his office by hereditary title and thus does not need to be reelected. Even under such a powerful executive, the government of France is limited by public opinion and is thus "essentially republican" (1:129). In making this comparison, however, Tocqueville does not indicate precisely how far the United States should or could go in imitating it.

One obvious lesson is that the U.S. president should be allowed to acquire more legislative power, permitting a greater consolidation of powers in the executive (1:127-41). A second seems to be that the presidency, although it could never be transformed into a hereditary office, might be strengthened by extending its tenure beyond four years and eliminating reeligibility (as some presidential reformers of today suggest in supporting a single six- or eight-year term). Such a modification follows from Tocqueville's remark that the founders conceived of the executive as a check on legislative despotism, but "by introducing the principle of re-election they partly destroyed their work." Contrary to their intention, they created a system that "stops the wheels of government" every four years, substitutes "management and intrigue for patriotism," and increases the power of the majority (1:142-43). Thus, Tocqueville's ideal American executive is virtually a constitutional monarch, a kind of "patriot king" (Ketcham 1984), who does not merely check the power of Congress but replaces it as the primary law-making entity.

In addition to modifying the separation of powers, Tocqueville seeks to counteract the Madisonian principle of dividing the majority into a multiplicity of interests. He explains the defect of this principle in a passage that reads like a direct response to *Federalist* no. 10: "All the passions that are most fatal to republican institutions increase with an increasing territory, while the virtues that favor them do not augment in the same proportion. The ambition of private citizens increases with the power of the state; the strength of parties with the importance of the ends they have in view; but the love of country, which ought to check these destructive agencies, is not stronger in a large rather than a small republic. . . . [For] the inclinations and interests of the people are more diversified by the increase in population, and the difficulty of forming a compact majority is constantly augmented" (1:167). Tocqueville here affirms Madison's premise that extending the size of a republic multiplies the number of competing interests, thereby decreasing the possibility of a compact majority. But in contrast to Madison he regards this as a bad tendency because it weakens the patriotism or love of country that should restrain the ambition of private individuals and political parties (Schleifer 1980, 112-20). Ironically, then, Tocqueville seeks to strengthen the majority by making it more cohesive or compact. But he does so in order to create an internal restraint, the sentiment of patriotism, rather than to rely on the external control of interest checking interest. What Tocqueville seems to be groping for here is not only a more cohesive union but a notion of the *nation* or the *national will,* recognizing that genuine self-government requires a sense of national responsibility rather than the fragmentation and haphazard play of competing interests.

In historical terms, Tocqueville's view of the American constitutional system stands in opposition to the prevailing ideas of his time—the ideas of Jacksonian democracy featuring states rights and sectional divisions, unlimited territorial expansion, the weakening of the national government through executive veto and contempt for the judiciary, and the glorification of the common man. As an alternative, Tocqueville seeks a combination of the best elements of the anti-Federalist and Federalist traditions (Ceaser 1985, 661)—a view that encourages the virtues of local self-government and strong national leadership in a moderately sized and cohesive union. This vision, in Tocqueville's judgment, is the best way of transforming the rather weak system of checks and balances in the United States into something that resembles a regime, a coherent moral order that teaches citizens responsible liberty and the discipline of self-government.

Conclusion

Tocqueville's constitutionalism is a novel synthesis of old and new ideas, a prudent adaption of ancient constitutional principles to the conditions of modern democracy. It shows how the classical conception of a *regime of virtue* can be used to critically analyze the effects of democratic sovereignty, as well as to correctly understand and modify the American system of checks and balances. At the foundation of this theory is the insight that democratic citizens who exercise liberty with a sense of moral responsibility attain a nobility of character that shines with the brightness of virtue.

In the last analysis, however, Tocqueville recognizes that moral responsibility is something of an illusion. It rests on the belief that people are the masters of their fate and can find within themselves the courage and discipline to govern themselves by laws of their own making. Yet, as Tocqueville asks in a poignant question, "What can be done with a people who are their own masters if they are not submissive to the Deity?" (1:318). By raising this question, Tocqueville indicates that even the noblest conception of human freedom cannot be limitless. It requires the mind to "bow with respect before truths which it accepts without question"; it requires limits imposed by divine law.

Indeed, Tocqueville ultimately appeals to a "higher law" pointing to a notion of moral perfection beyond human freedom, to an end given by nature or ordained by God. While this "higher law" is loosely mentioned several times in *Democracy in America* (1:269, 288, 434), it is developed systematically only once, in a remote chapter of volume 2 entitled "Of Honor in the United States." There, Tocqueville claims that morality in most times and places is governed by what the dominant classes honor and dishonor, praise and blame, admire and disdain. Although these codes are almost entirely conventional, people usually accept them without question or hesitation. Yet even while they obey, Tocqueville says, "they feel by a dim but mighty instinct, the existence of a more general, more ancient, and more holy law" (2:242). This intimation of a higher law is "the natural order of conscience" or "the general necessities of mankind revealed by conscience to every man." It stands above all conventional laws and valuations and points to the realm of "mere virtue, which lives upon itself, contented with its own approval" (2:244-55).

This intimation of a higher law, which appears to be a residual form of Christian natural law, is the ultimate foundation of Tocqueville's constitutional theory. It points to a standard above the will of the majority as well as the humanly created but morally binding modern constitution. By grounding human virtue in the divine and natural order, it provides the moral compass for all Tocqueville's prudential judgments in the new world of modern democracy. We thus conclude that Tocqueville's constitutionalism is a grand synthesis of classical regime analysis, modern constitutionalism, and Christian natural law, intricately woven into an original and coherent whole.

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NETWORKS IN CONTEXT: THE SOCIAL FLOW OF POLITICAL INFORMATION

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We examine the effects of individual political preferences and the distribution of such preferences on the social transmission of political information. Our data base combines a 1984 election survey of citizens in South Bend, Indiana with a subsequent survey of people with whom these citizens discuss politics. Several findings emerge from the effort. First, individuals do purposefully construct informational networks corresponding to their own political preferences, and they also selectively misperceive socially supplied political information. More important, both of these individual-level processes are shown to be conditioned by constraints imposed due to the distribution of political preferences in the social context. Thus, individual control over socially supplied political information is partial and incomplete. Finally, these information-transmitting processes interact with the social context in a manner that favors partisan majorities while undermining political minorities.

Politics is a social activity imbedded within structured patterns of social interaction. Political information is conveyed not only through speeches and media reports but also through a variety of informal social mechanisms—political discussions on the job or on the street, campaign buttons on a friend's shirt, even casual remarks. Such political information is processed and integrated not by isolated individuals but rather by interdependent individuals who conduct their day-to-day activities in socially structured ways and who send and receive distinctive interpretations of political events in a repetitive process of social interaction. Thus, political behavior may be understood in terms of individuals tied together by, and located within, networks, groups, and other social formations that largely determine their opportunities for the exchange of meaningful political information (Eulau 1986). In short, *environment* plays a crucial role in affecting the social flow of political information.

People choose their friends and the content of their conversations, but each of these choices is, in turn, bounded by an environment that, for many purposes, must be taken as given. We explore the ramifications of these two-way streets between individually determined friends and discussion content on the one hand and the socially structured supplies of possible friends and conversation content on the other. Hence our focus is on the content and perception of political information and the influence of the social context within which such information is exchanged. We turn initially to an examination of individual control over the social flow of political information before considering the role of the environment in limiting such control.

Individual Control Over Information

Political information is conveyed through social interaction, but does political preference determine the supply of information, or does the supply of information determine political preference? The direction of influence undoubtedly runs both ways. People choose information sources subject to their own preferences, but they also take what is available. They consume what social structure and social situations supply albeit guided in acceptance by individual perceptual mechanisms (Jones 1986). Thus, several different individual-level processes might account for the high levels of political homogeneity so frequently reported in studies of associated individuals.

High levels of correspondence between individual preference and the surrounding preferences of associated individuals might arise from discretion in the choice and use of associates. Individuals certainly choose to associate with some individuals and to avoid others, and they talk politics with some associates but avoid discussions with others. To the extent that these choices are motivated by a priori political preference, a high level of homogeneity in the politics of associates is the consequence of preference rather than its cause.

Indeed, the dominance or precedence of political preference over social influence corresponds well with a choice-theoretic perspective on political behavior. In his *Economic Theory of Democracy*, Anthony Downs argues (1957, 229) that it is fully rational for individuals to reduce information costs by obtaining political information from personal contacts. Among the benefits of socially obtained information is the ability of the receiver to choose the information source and thus to select a set of biases and viewpoints on the part of the source that the receiver finds congenial: "It is often relatively easy," writes Downs, "for a man to find someone he knows who has selection principles like his own."

A second source of reported political homogeneity within informational networks arises from measurement procedures. The level of correspondence between a respondent and associates is often determined on the basis of the respondent's own report. A single respondent provides information not only on his or her own political preferences but also on the political preferences of associates. Thus, if individuals selectively screen the political information they receive through social interaction, they may also systematically misperceive its content (Lauman 1973). Political homogeneity produced in this fashion is, from a purely technical perspective, a measurement artifact. Notice, however, that this is a measurement artifact with significant theoretical consequences. It indicates that disagreeable interactions can be reinterpreted (misperceived) in an agreeable light. Even if individuals are unable to control the objective content of socially transmitted political information, they are still able to control the interpretation of that content in a manner that reduces political dissonance, and thereby minimizes the impact of social influence.

Finally, additional subtleties may operate as well. An individual who is well aware of an associate's political preferences and also knows them to be at variance with his or her own beliefs may very well misrepresent, not fully communicate, or deliberately make ambiguous his or her own preferences precisely in order to reduce dissonance in the relationship (Macoby, Matthews, and Morton 1954). Hence, a discussion partner may be quite prepared to misperceive political information when, simultaneously, the transmitter is prepared to obfuscate the message (Jones 1986). It is not only political elites or candidates for public office (Shepsle 1972) who may have motives to heighten communication ambiguity in democratic politics (Calvert 1985; MacKuen n.d.).

In summary, three individual-level factors undermine the potential impact of socially transmitted information. Citizens choose with whom to discuss politics, they reinterpret dissonance-producing information, and they may deliberately misrepresent their true opinions. These individual-level capabilities bring us full circle. Political information is conveyed through social interaction, but individuals choose with whom to interact, and they also suppress dissonance-producing information if it is not already rendered politically inoffensive by the transmitter. On these premises, socially transmitted political information becomes, wholly and completely, an extension of individually based political preference. The potential influence of socially transmitted information is explained away on the basis of choice, misperception, and misrepresentation: sociology gives way to economics and individual psychology.

This position may be juxtaposed with a view that makes a case for the significance of social structure in determining the social transmission of political information. Not only is it worthwhile to inquire into the structural factors that motivate people to conform or to misrepresent their positions, but one might also examine the manner in which social structure determines interaction opportunities or even imposes interactions.

Social Control Over Information

A case for social influence might be made on either theoretical or empirical grounds. Anticipating empirical support for social influence we ask, Is there a theoretical basis from which to build an expectation for the influence of socially transmitted information?

First, to argue that the selection of information sources takes precedence over social influence is to assert that political preferences dominate social-influence processes running in the other direction. It is a gross simplification to assume that individually defined interests translate directly into individual preferences (Lewis-Beck 1986, 342-44), for preferences must be informed, and information arrives through socially structured channels. In particular, as McPhee (1963) persuasively argues, political preferences are sustained within particular structural locations, and they are configured by the multiple interactions of the individual within those structural locations. Thus, to the extent that political preferences are dependent on (socially supplied) information, they are inherently dynamic and responsive to social influence. Preference is not only inflicted upon socially transmitted information, but socially transmitted information is inflicted upon preference. Mutual causation between social contexts and individual preferences is inherent in this view of mass political-information processing.

Second, it is often difficult for political scientists to remember that politics is not at the forefront of most choices that most individuals make. Assuming for the moment that individuals do indeed choose their own locations within the social structure, the choices are likely to be predicated on matters other than politics and political preference (Brown 1981). People choose a job because it pays better or because they need work. People choose a neighborhood because they can afford one of the houses. People choose a church because their parents raised them in it. The point to be emphasized is that politics and political preferences are ancillary to most of the significant life choices people make, the choices that locate individuals within the social structure. But these choices, made largely on nonpolitical grounds, may nevertheless impinge mightily on politics. Particular locations within the social structure expose citizens to particular social contexts and the information biases such contexts willy-nilly provide. These social contexts circumscribe the opportunities for social interaction. Thus, individual choice regarding associational patterns and political discussions may be seen as operating within the opportunities and constraints imposed by a given social context.

These processes are stochastic. It is possible to buy into an upper-class neighborhood and find oneself surrounded by Democrats, but a preponderance of Republican neighbors is more likely. Individuals probably reason over these bundles of probabilities in a similar fashion: If I buy into this neighborhood, the chances are my daughter will date good Catholic boys (and ultimately marry one), the reasoning might run. But recognizing the contingent nature of the processes of living does not make the (stochastically determined) impact of events any less. Indeed, events that are not certain are more effective conditioners than those that occur regularly, as Skinner (1938) demonstrated long ago.

This is not to say that individuals are powerless in deciding with whom to discuss politics. In her study of a predominantly Democratic automobile plant, Finifter (1974) demonstrated that Republican autoworkers became socially introverted, resisting association with the numerically superior Democrats. In his study of a working-class suburb, Berger (1960) demonstrated that the middle-class minority was excluded and withdrew from community life. It is not that choice is absent, but rather that associational choice is contingent—a locationally specific response to a particular social mix.

The role of the mass media may be deceptive in regard to these social processes. The nationalization of political-information sources achieved through the disappearance of multiple-newspaper cities and the dominance of television may very well enhance the coercive power of informationally biased social contexts. If individuals in one-newspaper and three-look-alike-network cities are to hear second opinions, the only recourse is probably socially transmitted new information. But this will be biased by its social location, that is, by the social context. Hence, readily available political information from the modern mass media may, because of its increased uniformity and homogeneity, increase rather than decrease the political influence potential of the social context (Laurily K. Epstein, personal communication, 1986).

Evidence will help in assessing the substantive significance of the various processes we have sketched. We turn now to empirical matters with some analyses taken from a study with a research design intended to cast light on the sorts of issues just raised.

Networks and Contexts in South Bend

As a first step, it is crucial to draw an important, if abstract, theoretical distinction between *contexts* and *networks:* contexts are structurally imposed while networks are individually constructed. Contexts are external to the individual even if the composition of the context depends upon the makeup of individuals contained within it. In contrast, networks represent the product of myriad choices made by people who compose the net, but choices that are circumscribed by the opportunities and constraints imposed by context. To present an extreme example, a Mondale voter who resides in a context where all other individuals support Reagan will be unable to discuss politics with another Mondale voter. As a practical matter, this distinction between networks and contexts may blur, but it provides a useful analytic tool with which to consider the issues raised above.

The analyses are based on a study undertaken in South Bend, Indiana during the 1984 presidential election. The South Bend study was designed with a particular motive: to locate mass political behavior—political attitudes, candidate preferences, party preferences, policy positions, and political activities—within the social patterns of everyday life. Two structural ingredients are especially germane here: the informational networks through which individuals send and receive political information and the larger but still very immediate contexts within which individuals reside. Both networks and contexts were observed and measured as part of the South Bend study, and clarity about measurement procedures is important for understanding our results.

A three-wave panel survey of approximately 15 hundred respondents was conducted during the summer and fall of 1984 before, during, and after the presidential election. The respondents lived in 16 different South Bend-area neighborhoods purposely selected to maximize social homogeneity within the neighborhoods and social heterogeneity between the neighborhoods. These neighborhoods constituted the primary sampling units, and a high rate of random sampling was achieved in each neighborhood. The neighborhoods vary from extremely affluent planned suburban developments through respectable middle-class neighborhoods, through well-maintained working-class and ethnic enclaves, to poor and deteriorating central-city neighborhoods. All the neighborhoods are composed of predominantly white populations, and the small number of black respondents are excluded from the analyses reported below. (More detailed information regarding study design, response rates, and related issues is available upon request from the authors.)

Approximately equal numbers of respondents, in turn, were randomly selected within each of the neighborhoods and interviewed through the computer-assisted telephone interviewing facility of the Indiana University Center for Survey Research. Intensive sampling within neighborhoods produces two distinct advantages. First, any survey item can be aggregated to produce a measure of neighborhood context; thus, we can construct survey-derived estimates of neighborhood support for Reagan in the 1984 election. Second, intensive sampling makes it possible to employ procedures developed by Boyd and Iversen (1979) for the analysis of contextual data; we employ a variation of the Boyd and Iversen centering procedure to produce approximately orthogonal explanatory variables.

Contexts are measured at the level of the neighborhood, using an aggregated survey item to provide the contextual measure, and the relevant individual-contextual property is voting behavior in the 1984 election. Thus the context of political behavior, as distinguished from the network of political discussion, is defined geographically and socially on a priori grounds and measured directly from survey respondents. While our measure of context is the geographically based neighborhood, the social context is clearly a much broader concept that encompasses wider domains, such as religious affiliation, recreational activities, and social organizations. Thus the measurement employed here is narrower (and more conservative) than the theoretical construct it represents. Indeed, more comprehensive measures of social contextual experience would likely demonstrate even larger effects than those reported in our tables.

Networks are measured from two vantage points: first, that of the main respondent to the survey; second, that of the political discussant named by the main respondent. Respondents to the third-wave survey, conducted shortly after the 1984 election, were asked to give the first names of three people with whom they were most likely to have discussions about politics. If respondents could not name anyone with whom they discussed politics, a follow-up question asked for the first names of three people with whom they were likely to have informal conversations. The vast majority of the respondents were able to identify political discussants. The nature of the probe is very important because it was intentionally structured around a clear political reference point. We did not ask our main respondents to give us a list of their best friends or their close associates. Rather, we asked them to tell us with whom they discussed politics—their social sources of political information. Not surprisingly, many of the main respondents named discussants who were spouses and other relatives. However, for the purposes of this paper, all relatives by either blood or marriage are omitted; our focus is entirely upon unrelated discussants.

Notice that our definitions of *contexts* and *networks* do not require that the two overlap territorially. That is, a network may lie wholly outside the context, but this fact would not negate interdependence between these two elements of social structure. A person who lives in a context surrounded by Reagan voters may assume that everyone votes for Reagan, even when encountering individuals outside that context. Furthermore, a person imbedded in a Reagan context probably has a higher likelihood of encountering Reagan voters even when the encounter occurs beyond the context. Contexts create opportunities for social interaction which are not necessarily bounded by the original context.

A series of follow-up questions asked main respondents for information and perceptions regarding the discussants, including the presidential candidate for whom the discussant voted in 1984. A final question asked for the last name and street address of each discussant so that we could ask the discussants "a few short questions regarding the last election." The main respondents, perhaps surprisingly, were quite forthcoming with this information. Based upon their responses, the staff at the Center for Survey Research constructed a sampling frame of approximately 18 hundred discussants, and our interviewing budget allowed us to conduct a fourth interview with more than nine hundred discussants. In addition to these interviews, fifty-five main respondents reported a discussant who had also been interviewed in the third wave. This was not totally unexpected since the neighborhoods were small in size with a great deal of social integrity, and the sampling rate within the neighborhoods was accordingly relatively high. After the relationships between spouses and other relatives are removed the resulting dyadic data set includes about five hundred conversational relationships. Twenty-two percent of the relationships in our analyses involve repeated use of a main respondent—instances when a main respondent has more than one relationship represented in the data matrix.

Congruence and Dissonance Between Discussion Partners

The resulting data set offers a unique opportunity to examine political choices as they are imbedded within the social flow of political information. As a first step we examine (1) the objective correspondence between the preferences of main respondents and their discussants, (2) the perceptions of the political preferences of discussants held by main respondents, and (3) the accuracy of these

Table 1. Proportion of Main Respondents Who Correctly Perceive Discussant's Voting Behavior

	Discussant		
Main Respondent	Nonvoter	Reagan Voter	Mondale Voter
Nonvoter	.222 (9)	.790 (19)	.818 (11)
Reagan voter	.400 (20)	.912 (170)	.662 (65)
Mondale voter	.333 (15)	.547 (53)	.922 (90)

Note: Omits relatives. Percentage of Reagan voters' relationships that produce agreement = 170/(20 + 170 + 65) = 67%. Percentage of Mondale voters' relationships that produce agreement = 90/(15 + 53 + 90) = 57%.

perceptions. Table 1 summarizes this information for the relationships included in this analysis.

Voters with similarly voting discussants show a remarkably high level of accuracy in their perception of discussant behavior: 91% of Reagan voters with Reagan discussants correctly identified discussant behavior, and 92% of Mondale voters with Mondale discussants made similarly correct identifications. Accuracy is reduced somewhat within relationships that produce disagreement. Both Reagan and Mondale voters have considerably more difficulty in correctly identifying nonvoting discussants, but 66% of Reagan voters correctly identify discussants who voted for Mondale, and 55% of Mondale voters correctly identify discussants who voted for Reagan. This general pattern changes among the nonvoting respondents: they do not accurately identify fellow nonvoting discussants (22%), but they are fairly accurate in their identification of Reagan voters (79%) and Mondale voters (82%). Finally, Table 1 shows the general levels of agreement within relationships: 67% of the Reagan voters' relationships involve discussants who voted for Reagan, and 57% of the Mondale voters' relationships involve discussants who voted for Mondale.

Magnitude in political science is, like beauty, in the eye of the beholder, but Table 1 suggests several things to us. First, these relationships are not overwhelmingly homogeneous in terms of voting preference. They include substantial levels of political disagreement measured in the metric of reported votes. Second, while perceptual accuracy generally increases in politically congruent relationships, clear majorities of voters in dissonant relationships recognize the disagreement.

What factors are responsible for objectively defined congruence and dissonance within the relationships? What factors are responsible for accurate and inaccurate perceptions?

The Structural Basis of Political Information

In the analysis that follows we consider the preferences of discussion partners from two different perspectives: the perceptions of those preferences by the main respondents, and the objective reality of those preferences as they are measured by the discussants' own self-reports. The analysis comes in three question-stages: (1) Are the main respondents' perceptions of discussion partners affected by the political preferences of, and by the political preferences surrounding, the main respondents? (2) Are main respondents likely to have discussion partners whose objectively defined (self-reported) preferences correspond to the political preferences of, and the political preferences surrounding, the main respondents (in this instance the social context becomes important as it affects the supply of particular political preferences and, thus, the opportunities for discussion with people holding particular preferences)? and (3) Do the main respondents systematically *misperceive* discussant preferences as a function of their own preferences and as a function of the preferences surrounding them?

The relationships between networks, contexts, and individual preference are estimated using a logit model for micro data (Hanushek and Jackson 1977). The model is appropriate to these data where the dependent variables are binary—Reagan discussant or not, perceived Reagan discussant or not. An otherwise straightforward analysis is complicated by two factors. First, the choice and perception of discussion partners is one part of an inherently simultaneous social-influence process. A citizen's own political preferences influence the choice of a discussion partner, but this choice has subsequent consequences for the citizen's preference. These two phenomena are inherently dynamic and interdependent. The resulting technical problem is simultaneity leading to identification problems in the statistical models to be estimated. Discussant choice may be a function of individual preference and individual preference may be a function of discussant choice. Although we do not carry out the analysis for the implicit full simultaneous system, it is appropriate to take the simultaneity into account in estimation.

The simultaneity problem is addressed in the analysis that follows through the construction of two instruments that measure the likelihood that any individual (1) voted for Reagan or (2) did not vote. Each instrument is constructed from the logistic regressions shown in Table 2. Thus, rather than using dummy variables to represent the main respondents' voting behavior, we employ the probabilities estimated by the instruments (Maddala 1983). The instruments avoid the measurement bias due to simultaneity. They are constructed to correlate highly with reported behavior but to be (relatively) independent of discussant behavior (Wonnacott and Wonnacott 1979).

The second estimation problem arises due to the danger of potentially omitted explanatory factors. Beginning at least with the work of Hauser (1974), critics of contextual research have demanded that alternative, individual-level explanations be taken into account before asserting a contextual effect. In principle, this is laudable, but in practice it has often meant the inclusion of long lists of correlated individual-level control variables (Kelley and McAllister 1985). It should come as no surprise that the inclusion of highly correlated explanatory variables often weakens statistical purchase. That is, additional individual-level controls run the very real danger of producing excessive colinearity and thus a misplaced willingness to accept null hypotheses.

The research design for the South Bend study provides at least a partial solution to this second problem. By centering the individual-level variable around its mean within contexts and by centering contextual means around the mean for the sample as a whole, orthogonality is produced between the centered individual measure, the centered neighborhood mean, and their multiplicative interaction. This does not guarantee orthogonality between these variables and the additional individual-level controls, but it decreases colinearity to a degree sufficient for the

Table 2. Construction of Instruments for Main Respondents' Voting Behavior

Independent Variable	Probability of Reagan Vote	Probability of No Vote
Constant	-4.46 (4.66)	.92 (.67)
Age[a]	.01 (1.38)	-.05 (3.89)
Education[b]	.13 (2.37)	-.32 (3.72)
Party identification[c]	.83 (12.12)	—
Partisan loyalist[d]	—	.49 (2.68)
Interest[e]	—	1.50 (6.32)
Number of cases	506	506

Note: Logit model. *T*-values are shown in parentheses.

[a]Main respondent's age in years.

[b]Education of main respondent (years of schooling).

[c]Seven-point party-identification scale (0 = *strong Democrat*).

[d]0 = *strong partisan*; 1 = *weak partisan*; 2 = *independent leaning toward a party*; 3 = *independent*.

[e]Interest in election: 1 = *a great deal*; 2 = *some*; 3 = *only a little*; 4 = *none at all*.

present analyses. (See the Appendix for technical details.)

To summarize, the main respondent's voting behavior is measured through the use of two instruments that measure the likelihood of voting for Reagan and the likelihood of not voting. This removes the taint, technically, of causation from the discussion partner's behavior in statistical estimates of coefficients. A mean likelihood of voting for Reagan is then calculated within the neighborhood and for the sample as a whole. The individual-level Reagan vote instrument is centered around the respective neighborhood mean; the mean Reagan vote likelihood within neighborhoods is centered around the overall sample mean; and an interaction variable is formed from the product of these two variables. Finally, two demographic controls for income and education are included in each model as well.

These procedures are all well-understood and straightforward applications of statistical technique. They have the unfortunate side effect of appearing as a great deal of hocus-pocus; thus, we include a second model in each analysis that uses simple and straightforward procedures: no instruments, no centering, no interaction, no individual-level controls, where the main respondent's right-hand-side explanatory voting behavior is measured through the use of dummy variables. This simplified model corresponds to Alwin's formulation for estimating the effects of social context (Alwin 1976). We are not surprised (and the reader should be reassured) to find that the results are much the same for both models.

The centering procedure is best accomplished by calculating neighborhood means from the sample that is employed for a particular analysis. This means that, on average, the neighborhood means are calculated from approximately 24 interviews within each neighborhood. The reliability of the neighborhood means is supported by their very high correlation ($r = .95$) with the proportion of the entire third-wave sample in each neighborhood voting for Reagan. And in this latter case, the average sample size in each neighborhood is more than 85. The general reliability of the survey-derived contextual measures is further supported by the correlation of third-wave neighborhood demographic means with demographic measures for the neighborhoods taken from the 1980 census. Once again, the survey measures do quite well, correlating with the census measures at the .9 level.

A final measurement issue involves response bias in the survey. Our respondents report a level of voting turnout that is in excess of the true population value. This is due in part to common overreport problems, but it is also due to the panel design. People who agree to participate repeatedly in a panel study concerned with politics are more likely to be upper-status people, and they are also more likely to be voters. Thus, the question arises whether using neighborhood means provides a reliable measure of Reagan support within the neighborhoods. If the measure is intended to provide a precise estimate of the neighborhood Reagan support to the base of all eligible voters, then it certainly fails the validity test because turnout is undoubtedly overestimated. But this bias does not render the measure useless for present purposes. What is central to the analysis is variation in Reagan support across neighborhoods, and the measure is appropriate for such use. As we showed above, survey-derived measures of neighborhood social demographics compare very well with the census-derived measures of social demographics, and there is no reason to expect that survey-derived measures of political demographics would compare any worse. Thus, in spite of the overreport bias, there is strong reason to believe that it provides a good relative measure of Reagan support across neighborhoods.

Perhaps more importantly, the measure of neighborhood Reagan support has an additional virtue: it provides an estimate of Reagan support to the base of the politically active neighborhood population—the population that should matter both substantively and theoretically. That is, the survey ends up, de facto, being a survey of politically engaged citizens, and politically active citizens tend to be surrounded by other active citizens. Thus, from a substantive standpoint, the bias in the neighborhood means corresponds well to the bias in the sample. In terms of anticipated effects from the social context, this is a desirable design feature. The politically active population is the theoretically correct population to sample.

The Perception of Information

Does the perception of a discussant's preference continue to vary as a function of the main respondent's own preference when the social context is taken into account? Does the perception vary as a function of Reagan support in the main respondent's context?

Table 3 displays the results for the logit model that estimates the main respondent's perception of the discussant's vote as a function of both the main respondent's voting behavior and the level of Reagan support in the main respondent's neighborhood. Reagan support at both the individual and contextual levels generates coefficients that lie in the expected direction and possess respectable t-values. Respondents who support Reagan and (to a lesser degree) respondents who are less likely to vote, are more likely to perceive their discussants as Reagan voters than the respondents who support Mondale. Furthermore, respondents who live among Reagan voters are more likely to perceive their discussants as Reagan voters.

The magnitude of these effects are characterized in Table 4, where the probability of perceiving a discussant as a Reagan voter is computed from the model estimates reported in Table 3, across main respondent preferences and across the observed range of neighborhood Reagan support. In order to arrive at main respondent preferences, the mean value of both instruments is calculated within the three possible behavior categories of Reagan voter, Mondale voter, and nonvoter. The two demographic controls are held constant at the sample means. (See the Appendix for details.)

Table 4 shows that regardless of context, Reagan voters are most likely to perceive their discussants as Reagan voters and Mondale voters are least likely to do so. The table also shows that main respondents who reside among Reagan voters are more likely to perceive their discussants as Reagan voters, regardless of individual preference. This latter effect is substantial, and it compares quite favorably to the effect of individual

Table 3. Projected Reagan Vote for Discussant by Vote of Main Respondent and Reagan Support in Main Respondent's Neighborhood

Independent Variable	Instruments with Controls[a]	Original Data
Constant	-.40 (.52)	-3.15 (5.96)
Main-respondent Reagan support[b]	2.79 (7.97)	1.82 (7.78)
Non-voting main respondent[c]	.80 (1.08)	1.31 (3.51)
Neighborhood Reagan support[d]	4.58 (4.69)	4.04 (4.10)
Individual-context interaction[e]	3.88 (1.48)	—
Education of main respondent[f]	.003 (.06)	—
Income of main respondent[g]	.08 (1.06)	—
Number of cases	444	468

Note: Includes only discussants who are not relatives. Logit model. Dependent variable is projected Reagan vote. *T*-values are shown in parentheses. Projected Reagan vote = 1 if main respondent *thinks* discussant voted for Reagan; 0 otherwise.

[a] In this column, main-respondent Reagan support, neighborhood Reagan support, and their interaction are centered variables.

[b] The original variable equals 1 if main respondent voted for Reagan; 0 otherwise. The instrument varies from .05 to .98.

[c] The original variable equals 1 if main respondent did not vote for president; 0 otherwise. The instrument varies from .001 to .92.

[d] Mean level of Reagan support in main respondent's neighborhood, calculated from survey (mean instrument in left column; proportion Reagan voters in right column).

[e] Main respondent Reagan support multiplied by neighborhood Reagan support.

[f] Years of school completed by main respondent (possible range = 0–17; approximate mean = 13).

[g] Family income (possible range = 1–8; approximate mean = 5, or 20–30 thousand dollars per year).

Table 4. Probability That Main Respondent Perceives Discussant as a Reagan Voter by Main Respondent's Vote and the Proportional Reagan Vote in the Main Respondent's Neighborhood

	Neighborhood Reagan Support[a]	
Main Respondent's Vote	Low (.31)	High (.78)
Reagan	.38	.87
Nonvoter	.32	.76
Mondale	.21	.57

[a]These levels represent the range, on the neighborhood proportions, who reported voting for Reagan, for 1,390 nonmissing third-wave respondents. The corresponding centered neighborhood means for the Reagan support instrument, upon which the estimates are based, were -.21 and .24. Data from Table 3, col. 1.

preference. Differences of 17 and 30 points are shown between the perceptions of Mondale and Reagan voters, and the difference between main respondents living in Reagan and Mondale neighborhoods ranges between 36 and 49 points.

It is important to realize that the table displays something other than marginal effects. In an 80% Reagan neighborhood, if all that was involved was the increased supply of Reagan voters, the expected value should be the same—.8 for all three classes of voters. It is not. Indeed, the table shows either a discussant-selection bias based upon individual party preference—a consequence of personal preference imposed on social structure (Finifter 1974), or some form of misperception that is perhaps complex (Jones 1986), or both.

A common question looms behind both sets of effects. Do individual preferences and contextual properties affect the perception or the reality of the socially transmitted information? Put another way, Are these perceptions rooted in an accurate assessment of discussant preferences or in an interpretation that distorts discussants' actual (self-reported) preferences? We divide this problem into two components by (1) investigating the discussants' self-reported preferences before (2) examining the main respondents' perceptions in light of discussants' self-reported preferences.

The Content of Information

Is the objective content of socially transmitted information affected by individual preference? Is it affected by the social context? These questions are attacked indirectly by considering the discussant's self-reported vote (interpreted here as the objective political content of socially transmitted information) as a function of the main respondent's vote and the level of Reagan support in the main respondent's neighborhood. Our concern is not with the conditions that affect perceptions of discussant preferences but rather with the conditions that affect the reality from which these perceptions are developed.

The logit model of Table 5 estimates the choice of a Reagan voting discussant as a function of the main respondent's voting preference and the main respondent's social context. Essentially, the table answers the question, How powerful are your preferences and the preferences that surround you in determining the likelihood that you will have a discussion partner who reports a particular preference? The coefficients of central interest lie in the expected direction and each possesses a satisfactory *t*-value. Reagan supporters are more likely to have discussants who voted for Reagan, and main respondents living among Reagan voters are more

Table 5. Self-reported Reagan Vote for Discussant by Vote of Main Respondent, and Reagan Support in Main Respondent's Neighborhood

Independent Variable	Instruments with Controls[a]	Original Data
Constant	.28 (.41)	-1.69 (3.72)
Main-respondent Reagan support	2.11 (6.43)	1.03 (4.91)
Nonvoting main respondent	1.03 (1.48)	.52 (1.56)
Neighborhood Reagan support	2.72 (3.04)	2.34 (2.68)
Individual/context interaction	2.84 (1.13)	—
Education of main respondent	-.08 (1.46)	—
Income of main respondent	.16 (2.19)	—
Number of cases	466	491

Note: Includes only discussants who are not relatives. Logit model. Dependent variable is discussant Reagan vote. *T*-values are shown in parentheses. Discussant Reagan vote = 1 if discussant reports voting for Reagan; 0 otherwise.

[a] In this column, main-respondent Reagan support, neighborhood Reagan support, and their interaction are centered variables.

likely to have discussants who voted for Reagan. The table provides evidence for the selective choice of discussants based on the preferences of main respondents and also evidence for social coercion arising from the partisan supply available in the social context of the main respondents. It is not only the perception of discussant preferences that is affected by context but the reality underlying these perceptions as well. Not only are citizens who live in contexts dominated by Reagan supporters more likely to perceive their discussion partners as Reagan voters, but these citizens are also more likely to have discussion partners who *are* Reagan voters.

The magnitudes of these effects upon the probability of having a self-reported Reagan voter for a discussant are displayed in Table 6. As before, the effect of context compares favorably to the effect of individual preference, but the effects demonstrated in Table 4 are somewhat more pronounced. Reagan voters are 15 and 26 points more likely than Mondale voters to have a discussant who voted for Reagan. And in comparison to main respondents living among Mondale voters, main respondents who live among Reagan voters are between 22 and 33 points more likely to have a discussant who voted for Reagan. Thus, the objective political content of socially transmitted information (the true partisanship of the discussant) depends both upon associational choice on the part of the receiver and upon the constraints on supply arising from the political composition of the social context that circumscribes that choice.

Note that there is no reason to expect the partisan behavior of discussion partners to reflect the contextual distribution

Table 6. Probability That Discussant Voted for Reagan by Main Respondent's Vote and the Proportional Reagan Vote in the Main Respondent's Neighborhood

Main Respondent's Vote	Neighborhood Reagan Support Low (.31)	Neighborhood Reagan Support High (.78)
Reagan	.46	.79
Nonvoter	.44	.71
Mondale	.31	.53

Note: Data from Table 5, col. 1.

of partisanship if discussion partners are chosen only on the basis of the selector's individual partisanship. The latter condition predicts the same cell entries in both columns of a given row in Table 6, which manifestly is not the case. In short, then, the objective content of the partisan signal in the political discussion process is a function of both the individual's preferences and the preferences in the individual's surroundings. But does this make any difference? If misperception is sufficiently systematic and sufficiently strong, then objective content may not matter. We now turn to an investigation of this final issue.

The Perception and Misperception of Information

Is the misperception of socially transmitted information affected by the political preferences of the receiver? Is it affected by the social context? More importantly, does misperception reinforce individual political preference in the choice of discussion partners and thereby offset the potential of the social context to expose individuals to sources of dissonant information?

To address these issues, it is necessary to take account of the objective (self-reported) voting behavior of both the discussant and the main respondent when examining the main respondent's perceptions of the discussant's vote. The logic of the analysis is to investigate the main respondent's perception of the discussant's vote while controlling for the discussant's objectively defined (self-reported) vote. With objective content controlled, it is possible to assess whether individual misperception overwhelms the effect arising from information exposure to partisan distributions in the individual's social context.

Table 7 displays the logit model that is obtained when the main respondent's perception is treated as the left-hand-side dependent variable, and the self-reported voting behaviors of both the main respondent and the discussant, as well as Reagan support in the neighborhood, are treated as right-hand-side explanatory variables. The explanatory variables of central interest produce coefficients that lie in the expected direction with crisp t-values. In general, main respondents are more likely to perceive a discussant as a Reagan voter if the discussant voted for Reagan (objective reality), the main respondent resides among Reagan supporters (social reality), and the main respondent supported Reagan (subjective reality). The table shows that controlling for the discussant's objective partisanship does not eliminate either individual-preference effects or social-context effects on perception.

These results become more provocative when the magnitudes of effects are considered in Table 8. This table is striking

Table 7. Projected Reagan Vote for Discussant by Vote of Main Respondent, Self-reported Vote of Discussant, and Reagan Support in Main Respondent's Neighborhood

Independent Variable	Instruments with Controls[a]	Original Data
Constant	-2.33 (2.40)	-4.39 (6.52)
Discussant Reagan vote	2.81 (9.52)	2.94 (10.17)
Nonvoting discussant[b]	.49 (1.11)	.46 (1.08)
Main-respondent Reagan support	2.10 (4.95)	1.77 (6.00)
Nonvoting main respondent	.62 (.66)	1.39 (2.97)
Neighborhood Reagan support	4.20 (3.59)	3.45 (2.93)
Individual–context interaction	3.26 (1.02)	—
Education of main respondent	.07 (1.01)	—
Income of main respondent	-.01 (.11)	—
Number of cases	428	450

Note: Includes only discussants who are not relatives. Logit model. Dependent variable is projected Reagan vote. *T*-values are shown in parentheses.

[a] In this column main-respondent Reagan support, neighborhood Reagan support, and their interaction are centered variables.

[b] 1 if discussant did not vote; 0 otherwise.

Table 8. Probability That Main Respondent Perceives Discussant as a Reagan Voter by Main Respondent's Vote, and the Proportional Reagan Vote in the Main Respondent's Neighborhood

Vote of the Main Respondent and Discussant	Neighborhood Reagan Support	
	Low (.31)	High (.78)
Reagan-Reagan	.68	.95
Reagan-nonvoter	.18	.65
Reagan-Mondale	.12	.53
Nonvoter-Reagan	.64	.91
Nonvoter-nonvoter	.15	.51
Nonvoter-Mondale	.10	.39
Mondale-Reagan	.54	.84
Mondale-nonvoter	.10	.34
Mondale-Mondale	.07	.24

Note: Data from Table 7, col. 1.

Table 9. Probability That Main Respondent Correctly Perceives Whether or Not the Discussant Is a Reagan Voter by Main Respondent's Vote, the Discussant's Vote, and the Proportional Reagan Vote in the Main Respondent's Neighborhood

Vote of the Main Respondent and Discussant	Neighborhood Reagan Support	
	Low (.31)	High (.78)
Reagan-Reagan	.68	.95
Reagan-Mondale	.88	.47
Mondale-Reagan	.54	.84
Mondale-Mondale	.93	.76

Note: For main respondents and discussants who voted for Reagan or Mondale. Data from Table 7, col. 1.

for the probability differences it reveals between objectively homogeneous versus objectively dissonant dyads and, similarly, between politically supportive and nonsupportive social contexts. These results can be made even more vivid by focusing on only the dyads of central theoretical interest and by transforming the dependent variable. In Table 9 the dependent variable has been transformed into the probability of correctly identifying whether the discussant is a Reagan voter. Thus, a main respondent who perceives a Mondale voter as a Reagan voter is incorrect, as is a main respondent who perceives a Reagan voter as a nonvoter, and so forth.

Table 9 is worth some study. It shows, first, that voters tend to be accurate in their perceptions of discussion partners who agree with their own preferences: Reagan voters tend to recognize other Reagan voters, and Mondale voters tend to recognize other Mondale voters. This is especially the case when surrounding preferences are supportive of the voter's preference. In both instances the social context has at least a modest effect upon perceptual accuracy. But now examine disagreeing dyads.

Clearly, the greatest opportunity for contextual effects upon perceptual accuracy arises in discussion dyads that involve heterogeneous preferences. This is consistent with theory about the dynamics of the political discussion process (Huckfeldt 1986; MacKuen and Brown 1987; McPhee 1963; Sprague 1982). Reagan voters are more likely to perceive Mondale discussants accurately in Mondale contexts and less likely to perceive them accurately in Reagan contexts. Similarly, Mondale voters are more likely to perceive Reagan discussants accurately in Reagan contexts and less likely to perceive them accurately in Mondale contexts. Thus, majority versus minority status for a particular political preference in the social context emerges as a crucial consideration in cases where there is objective political disagreement between discussion partners (Noelle-Neumann 1984).

Finally, notice that the holders of a *minority* preference are more likely to perceive the *majority* preference accurately: they are more likely to perceive their discussion partner's political preference correctly if *disagreement* is present within the dyad. Thus, the social context transforms the nature of discussion dyads even between discussion partners who share the same preference. Members of the minority evidently come to expect political dissonance during social encounters and frequently fail to recognize political agreement even when it is present.

Interpretation

Two interpretive comments are central to understanding these results. First, a politically supportive social context sustains the receiver's misperception of dissonance-producing, socially transmitted political information. Second, members of the political minority accurately perceive members of the majority, while members of the majority do not accurately perceive members of the minority.

More analysis is necessary to sort out the nature of these contextual effects, but the substantive political consequences seem clear. Minorities are debilitated (Miller 1956) because they are often acutely aware of their own minority status. They are likely to recognize the variance between their own preferences and those of their surroundings. In contrast, and for whatever reason, dissonant information is frequently ignored by members of the majority.

In short, the majority enjoys a double benefit while the minority suffers a double liability. Not only are members of the minority more likely to encounter dissonance-producing information, they are also more likely to recognize it as such. Not only are members of the majority less likely to encounter dissonance-producing information, they are also less likely to recognize it as such.

Conclusion

The empirical analyses above emphasize the interdependence of private preference and politically relevant distributions in the individual's social context. The interesting conditions, as expected on the basis of theory, are those of dissonance—either between the main respondent and the discussant, or between the main respondent and the preponderance of political opinion in the respondent's social context. It is clear, especially from Table 9, that these conditions interact.

It would appear that individuals do purposefully attempt to construct informational networks corresponding to their own political preferences. This exercise of choice, however, is not independent from socially determined conditions of supply—from the contextually imposed opportunity (or lack of opportunity) for social interaction with people who hold various political preferences. Similarly, selective misperception is apparent in the analyses, and it, too, is socially conditioned. (These issues are addressed in Huckfeldt and Sprague, n.d.)

The most interesting feature of the empirical work, perhaps the most surprising result, and certainly the most significant for the operation of democratic politics, is the asymmetry in choice and perception arising from majority or minority political status in one's social context. Political majorities are able to ignore dissonant information. Political minorities, in contrast, suffer from a heightened level of vulnerability for the simple reason that their members accurately perceive the incoming flow of dissonant information. This analysis reveals, then, a process of informational coercion of political minorities achieved through mechanisms of social interaction.

Appendix

The analysis conducted here benefits greatly from the statistical framework for contextual analysis set forth by Boyd and Iversen (1979), but it also marks a significant departure from their model. (For another application of the Boyd-Iversen model see Esser 1982.) They develop their argument within the general framework of the linear model, but we adapt part of their centering procedure to this analysis using the nonlinear logit model. The differences are sufficient to warrant a brief explanation.

First, a great virtue of the Boyd and Iversen approach is that variance may be partitioned between individual effects, contextual effects, interaction effects, and error. Since the logit model has no error term, this is impossible in the current application.

Second, Boyd and Iversen develop some useful techniques to check for appropriate model specification that cannot be used in this instance. The root cause of this inability is, once again, the lack of an error term. In particular, we are unable to check for homogeneity of error across contexts, or for correlation between explanatory variables and error across contexts. An especially crucial test in the Boyd and Iversen model is the ability to recover equivalent coefficients for the two-equation and the single-equation specifications, but the two-equation specification cannot be estimated using the logit model. Thus, while we cannot carry out this check using the logit model, we have carried it out with the South Bend data for other specifications of the linear model with different dependent variables. These tests have been uniformly successful.

Our inability to carry out the Boyd and Iversen tests for misspecification is unfortunate, but it is not crippling, at least by contemporary standards. The vast majority of contextual studies, and indeed every empirical study, make assumptions regarding the behavior of error terms. The utility of Boyd and Iversen's approach is that it offers a way to check many of these assumptions, but few empirical studies are able to employ such a technology.

Finally, the Boyd and Iversen centering procedure not only transforms the explanatory variables, but the dependent variable as well, in order to maintain the original metric on the estimated coefficients. This cannot be done in the current instance; thus, the metric for coefficients is transformed. This is not a problem, however, because the metric for coefficients in a nonlinear model, such as the logit model, offers little interpretive insight. Instead of interpreting the magnitudes of coefficients, we use the logit model to calculate change in probabilities across values of one explanatory variable while other explanatory and control variables are held constant. As was stated in the text, social demographics are controlled at their sample means, and the instruments are controlled at their mean levels for the respective reported vote categories. The range on centered neighborhood means for Reagan support is calculated on the basis of the entire third-wave survey in order to take advantage of the larger sample size. The following outline shows the values for the controlled variables, as well as providing the central formulas for the centering procedure:

A. Centering procedure

Centered Reagan support at the individual level $= R_{ij} - \bar{R}_j$

Centered Reagan support within neighborhoods $= \bar{R}_j - \bar{\bar{R}}$

where

R_{ij} = the score of the *i*th respondent in the *j*th neighborhood on the Reagan support instrument

$\bar{R}_j$ = the mean score on the Reagan support instrument in the *j*th context

$\bar{\bar{R}}$ = the mean score on the Reagan support instrument for the sample

B. Control values used to generate estimated probabilities in Tables 4, 6, 8, and 9

1. Demographic means

Education = 13
Income = 5

2. Means for instruments across reported vote categories

	Reported Vote		
Instruments	Reagan	No Vote	Mondale
Centered Reagan vote probability	.18	−.08	−.26
Nonvote probability (not centered)	.06	.36	.09

3. The range on centered neighborhood Reagan support (calculated from the entire third wave)

highest neighborhood level of Reagan support = .24

lowest neighborhood level of Reagan support = −.21

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WHO WORKS WITH WHOM? INTEREST GROUP ALLIANCES AND OPPOSITION

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Interest-group interactions may be examined in ways comparable to the analysis of conflict and coalition in other areas of political science. We seek to measure and compare the structure of interest-group participation and conflict in four domains of U.S. domestic policy: agriculture, energy, health, and labor. Data are drawn from a survey of 806 representatives of organizations with interests in federal policy, supplemented by interviews with 301 government officials in the same four domains. Several types of data are adduced regarding the intensity and partisanship of group conflict in each domain and the range and variety of group participation. Coalitional patterns are described and the mutual positioning of different kinds of organization—peak-association groups versus more specialized trade, professional, or commodity groups, for example—are examined.

Some fields of intellectual endeavor enjoy the luxury of exploring well-defined problems whose difficulties, however intractable, are themselves part of a self-conscious culture of inquiry. Other fields of academic effort may well have substantial common ground, but its contours and possibilities have not yet been fully recognized by those committed to its exploration. One such field is interest-group politics. For two decades now we have gradually been coming to appreciate that the questions and concepts we had thought were central to this area of investigation were substantially misspecified and that quite another tack was needed if we were to make sense, theoretical or descriptive, of the burgeoning mass of activity to which we attach the label of *interest groups*.

Since Mancur Olson (1965) turned us in a different direction, we have been the beneficiaries of important work on the origins and maintenance of voluntary associations (inter alia, Berry 1977; Moe 1980; Salisbury 1969; Walker 1983). Concepts have been reformulated in order to encompass institutions within the interest-group frame of reference (Salisbury 1984). We know much more than we did before about lobbying tactics and strategy (Berry 1977; Schlozman and Tierney 1985). We

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have accumulated sophisticated treatments of both individual group and sector-level development and change over time (Browne and Salisbury 1972; Hanson 1985; Laumann and Knoke 1987; Starr 1982; Walker 1983). And we have useful cross-national comparisons of the structural ties among business, labor, and the state (Schmitter and Lehmbruch 1979; Wilson 1985). Here we propose to extend the range of our attention to a topic central to many fields of political science but not yet addressed by students of interest groups—the structure of conflict.

Whenever a more or less well-defined system of interactions can be observed or inferred—in legislatures, electoral politics, or international relations, to take three good examples—we move quickly to characterize that system in terms of the conflicts over scarce resources that have so long been held to be the defining feature of politics. We may focus on particular episodes—the struggle over a bill in Congress, a specific presidential contest, or the Falklands War. We may select out some feature of the overall structure for attention—the committee system, third-party challenges, or coalition patterns in the United Nations. We have developed quite a few fragments of theory regarding both generic conflict patterns and behavior conditioned thereby and the operation of specific institutional systems. It may be that the linkages between the more abstract and the more particular are sometimes weakly forged, but it seems plain that substantial progress has been registered by growth in cumulative understanding, theoretical and empirical.

We propose to argue that interest-group politics in the United States constitutes a system, or set of systems, defined by interaction among organized groups and between those groups and public officials. The structure of this system, and of subsystems within it, may usefully be characterized in terms of the extent and shape of conflict among participants. This structure is not evident in every instance of policy dispute, nor is it necessarily revealed by any particular case example of interest-group activity. But, as with voting studies or congressional roll calls, the interest-group system or systems can be discerned at more aggregated levels of observation.

The Research Design

It is ironic that it should have taken so long to get at the structure of interest-group conflict. Since the conceptualizations of Arthur Bentley (1908) began to work their yeasty way into political science, it has been commonplace to put group conflict at the very core of our thinking. Having done so, however, we have been so far unable to go beyond examples to reach more comprehensive formulations of how group interactions are structured. Is there much conflict or little? Are the main group players aggregated into two opposing camps or fragmented among several "parties"? Does the structure vary from one policy area to another? We cannot speak to these questions from existing literature. Recent work has greatly improved our understanding of how groups are formed and what they do, but it has not addressed the structure of conflict (for a partial exception, see Schlozman and Tierney 1985, 283-88). Part of the reason may be the absence of any readily observed defining behavior. Party systems can be examined by looking at the distribution of votes. So can legislatures. So can the Supreme Court. The executive branch is more difficult because criterion actions that could be the basis for characterizing their relationships are not visibly undertaken by all participants. Interest groups presented the same difficulty. No set of observations of group activity with enough scope of coverage or iterations of behavior has been made to allow reasonable inferences to be drawn regarding the existence and shape of systemic structure. Our data overcome these difficulties.

The design of our research began with the construction of lists of organizational participants in each of four policy areas—agriculture, energy, health, and labor. It was our firm belief that unless we linked our analysis of interest representation to the substantive policy concerns of the groups involved we would have a bloodless set of responses, interest-group activity without interests. In order to secure enough respondents in a given policy area to enable our analysis effectively to incorporate these substantive considerations, it was necessary to limit ourselves to a few policy domains. Our selection was bound to be somewhat arbitrary, but we have sought to tap some of the important dimensions of interest representation and to capture a reasonable portion of the range and diversity of representational activities.

The policy domains we examine exhibit very different patterns of institutional and policy change over recent decades. In agriculture and labor there has been a relatively stable set of executive agencies and congressional committees and subcommittees; and the programs in these fields, while not without controversy, have displayed substantial continuity in general design. Health and (especially) energy, by contrast, have undergone much greater changes. The past decade has seen each come under the jurisdiction of a new or redesigned cabinet-level department. Congress has developed new mechanisms to deal with energy issues, and the issue agendas of these two fields have been in great flux.

Both level and form of conflict were expected to vary. Labor policy typically pits two broad constellations of interests—business and labor—against each other, and the struggles are sometimes of epic proportions. Health matters, by contrast, were once the focus of great struggle, but, although health-care-cost containment continues to inspire sharp disagreement, most health issues in recent years have attracted quite muted political voices. Apart from the labor field, we expect to find a general tendency toward interest proliferation and fragmentation as the interests of various types of specialized producers—doctors, insurance companies, medical schools, and so on in health; oil and gas, coal, nuclear, and other fuel interests in energy; various crop and commodity interests in agriculture—grow increasingly differentiated. To be sure, there are peak associations in some domains—agriculture and health, as well as labor—but no organization can claim even this status in energy, and in the other fields the hegemony of the American Medical Association or the American Farm Bureau Federation has been badly eroded in recent years.

We expect our domains to display diversity, therefore, in the patterns of interaction among organizational actors, and we will be able to investigate to what extent this diversity reflects differences in the substantive policy and organizational composition of the domains. Alternatively, we can estimate how much of what individual-interest representatives *do* reflects their personal backgrounds and present job circumstances and how much is, in effect, imposed by the contemporary context of political and institutional forces within which interest representation must operate.

Our selection of policy domains is skewed toward domestic policy. Issues of foreign trade and other international concerns do arise, but they are not of central concern in any of the domains in our study. Interest representation on domestic matters may be larger in volume and differently structured than on matters of national defense, foreign aid, and other issues involving international considerations. Even those differences are hardly total, however, and we would hope that our findings sufficiently reflect the bulk of the policy process to be of some relevance to the understanding of private-interest representation well beyond the four domains of policy examined.

The procedures employed to identify organizations active in a particular policy domain required substantial documentary investigation and preliminary interviewing. No existing list adequately defines the population of interested participants. Each policy area includes several subsystems that attract somewhat different types of participants, and any single method of locating participating organizations has a systematic bias toward identifying certain kinds of groups and neglecting others (Salisbury 1984). We used four different listings of organizations for each policy domain, and the resulting rosters are broadly inclusive not only of the highly active groups but also of the marginal and intermittent participants. The sampling methods are more fully described in the Appendix.

Having defined the universe of group participation (and welcoming the natural weighting that resulted from multiple nominations), we drew a sample of 311 organizations, equally distributed across the four domains. By telephone we interviewed an appropriate official from each, asking, among other things, who represented their interests in Washington. Each group could name up to four individuals within the organization and up to four people from outside. These nominations (totaling 1,716 individuals) then became the basis for sampling respondents. A total of 774 were interviewed in person, a response rate of 77%. Interviews averaged about 75 minutes in duration. These data form the heart of our research. We asked each respondent to name three government officials with whom he or she regularly interacted and then interviewed 301 of those officials. Finally, we added 32 persons who had been among 72 nominated by informants as notable participants in the four policy domains but who, unlike the other 40, had not appeared in our sample.

Some of the analysis that follows treats respondents as individuals constrained by operating within a policy domain and other factors as well but giving their personal reactions and estimates. In some of the analysis, however, we let the individuals stand for the organizations that employ them and treat the organizations as the actors. This is appropriate for purposes of examining the patterns of organizational conflict, but it generates one unanticipated and unfortunate side effect. Approximately 20% of our respondents are external lawyers and consultants. Each was nominated by an organization, but in a fair number of instances they work also for other clients. We found that when asked about allies and adversaries or about actions and attitudes relating to specific events, these respondents gave replies that could not dependably be assigned to the nominating organization. Too often we could not tell what interests were reflected. Consequently, we determined to treat external lawyers and consultants as employees of their respective firms and not try to include them in the ally-adversary structures.

Quite a few of the larger organizations are represented by more than one respondent in our sample; indeed, there are as many as four from the AFL-CIO, the chamber of commerce, and a few others of comparable significance. There are also some state and local chapters or branches of national organizations, which we treat as part of the same category as the parent group.

We turn now to our empirical findings regarding the structure of group conflict in the four policy domains. We have three distinct sets of data bearing on the general question. First, for both Washington representatives and government officials, we report perceptions of conflict in each domain. We compare domains to see what differences appear in these perceptions and also examine some likely indicators of individual variation in perception. Second, we look at the patterning of alliance and opposition as revealed by responses to questions asking interest representatives to nominate other groups they worked with or considered as adversaries. A third set of data, to which only brief reference will be made in this paper, is drawn from responses to questions regarding actual participation in each of some 20 very specific events that occurred during the five years prior to our interviews in 1983–84. If a group had been active and taken a particular position, it could then be located, through smallest-space analysis, in relation to all the other groups active in that domain. From these quite different data sets we derive reasonably consistent pictures of the extent and shape of group conflict in each domain. In turn, we can compare those structures and draw inferences regarding both policy-domain characteristics and, more generally, the character of interest-group conflict in the United States.

The Perception of Issues

As we attempt to assess the nature of interest conflict in our four policy domains, it will be helpful to have some idea of how the interest representatives themselves characterize the issues they deal with. In this section we examine three dimensions: *stability, scope of participation,* and *intensity of conflict.* Our data are drawn from responses to pairs of opposing statements. Respondents placed themselves on a five-point scale, but in the presentation that follows, rather than treat them as interval data, we simply subtract the percentage of *disagree* responses from the percentage of *agree* choices. In no instance was there any discernible bimodality of responses; thus we are satisfied we have not lost or obscured anything substantial in opting for this simple procedure. The data are presented in Table 1.

We present two measures of issue stability, one on the long-livedness of issues in the domain and one on the stability of group coalitions. A large majority in each policy area sees the issues as long lasting, and both government officials and interest representatives share this view. Indeed, the agreement is sufficiently great that it might be accepted as settled that most of the business of policy-making entails working and reworking issues with a long history. Not many "new" items get on the agenda, and few matters are ever truly disposed of with dispatch.

There is far less agreement regarding the stability of issue coalitions, though in each domain a modest majority perceives stability rather than flux. The size of that majority is reasonably consistent across policy domains, though higher in labor than elsewhere. Except in the health field, government officials perceive substantially less stability than do interest representatives. We will explore the possible meaning of these differences after we have reviewed the other two sets of issue characteristics.

To assess the scope of interest-group activity in each domain, we asked respondents to estimate the amount of attention given by the general public to domain issues and the relative number of groups actively interested in them. Two points can be made. First, the energy field is seen as considerably broader in participation and attention than the others. Second, agriculture is seen as the narrowest. Doubtless this reflects the excitement surrounding energy issues in the period of our inquiry, but it may also be true that there is a particularly wide array of substantive concerns in this domain. It is interesting that energy representatives offer such high scope estimates. In the health field the representatives give much

Table 1. Perceptions of Issue Characteristics (in Percentages)

Domain Issue Characteristics	Agriculture		Energy		Health		Labor		Total	
	Representatives	Government Officials	Representatives	Government Officials	Representatives	Government Officials	Representatives	Government Officials	Representatives	Government Officials
Issue stability										
Issues on long-lived	68	52	78	88	71	59	68	56	72	64
Coalitions are stable	26	5	21	17	28	28	35	19	27	22
Scope of domain participation										
High public visibility	38	47	78	65	43	65	54	63	53	60
Large number of actors	17	35	52	53	33	61	34	47	34	49
Level of domain conflict										
High intensity of conflict	49	6	70	88	44	45	70	69	48	67
Strong partisanship	-18	-11	11	31	-7	3	37	46	4	15
Number of respondents	189	74	181	80	214	73	190	74	774	301

Note: Figures are the percentage of positive responses minus the percentage of negative responses.

lower figures than the government officials. This suggests that the health representatives are more substantively specialized than the officials, and hence encounter a smaller slice of the domain's business. In energy, it appears, this distinction is not present. Energy politics have been in such turmoil that it is not surprising that both officials and representatives have been exposed to a broad scope of activity.

A third dimension of domain-issue characteristics involves the degree of group conflict. We asked about the intensity of conflict in general and also about the extent to which domain issues were contested in partisan terms. The two questions draw sharply different responses—conflict is generally said to be high and partisanship to be low. Government officials rather consistently perceive somewhat greater conflict than do private-interest representatives. As one moves from one domain to the next, however, responses on the two questions move more or less together and display rather startling domain differences. Agriculture and health are less conflict-dominated and much less partisan than energy and labor. The labor domain appears to be intensely partisan in comparison with the other domains, and this characteristic ought to appear in the data on allies and adversaries as well. Partisan conflict should be expressed as essentially bipolar opposition whereas the ubiquitous but less partisan disputes of the energy realm might be expected to take a more dispersed, pluralistic form. When we examine the ally-adversary data, we can see whether this expectation is confirmed.

In an effort to probe more deeply into the sources of variation in the issue perceptions of individual representatives, we undertook an extensive probit analysis, employing a considerable number of independent variables. The results were not sufficiently robust to warrant their full presentation here, but they did generally support one important conclusion. We employed several different indicators of individual characteristics, including organizational position, prior government experience, present degree of policy representation effort, and personal values. A contrasting set of considerations consisted of the policy domain involved and the substantive type of organization represented, including labor unions, farm groups, and so on. In virtually every case the latter considerations had much more substantial effects. Agriculture is quiet and nonpartisan; labor is noisy, conflictual, and partisan. Energy and health are highly visible with many participants, while agriculture is perceived to be otherwise. Peak-association representatives see themselves more embroiled in controversies of broader scope and attention (apart from the actual range of their issue involvement) than do those who serve more specialized interests. These differences make reasonable sense and give support to our hope of capturing some diversity by our choices of policy domains.

Allies and Adversaries: Who Works with (and against) Whom?

In this section we examine responses to two questions. First, we asked whether the organization employing the respondent regularly encountered other organizations, as either allies or adversaries, in the particular policy domain. If they answered affirmatively, we asked the respondent to name up to three of each. The responses allow us to chart in considerable detail the patterning of interorganizational relationships. We will distinguish between organizations that are deeply embroiled in group conflict and those that remain on relatively friendly terms with others in the domain. We will

Table 2. Nominations of Interest-Group Allies and Adversaries

Domain	Allies	Adversaries
Agriculture		
Total nominations	422	287
Organizations named	186	116
Nominators	161	133
Energy		
Total nominations	449	321
Organizations named	143	95
Nominators	165	142
Health		
Total nominations	497	244
Organizations named	215	96
Nominators	185	125
Labor		
Total nominations	446	349
Organizations named	184	99
Nominations	167	151

also examine how domains differ in their conflict structures and the extent to which those differences are embedded in the substantive differences among interests in different domains.

Overwhelming majorities in each domain are able to identify both allies (89.9%) and adversaries (74.8%). Labor is most completely defined in ally-adversary terms, health and agriculture somewhat less so. Responses do not differ much by organizational position, though government-affairs specialists are a bit more likely than others to name adversaries.

Probit analysis of the same kind referred to in the previous section indicates that indeed the labor domain is especially likely to generate adversaries. Moreover, labor groups in whatever domain they participate, are significantly more likely to identify adversaries than are business peaks, business corporations, citizen or externality groups, and energy producers. Time spent by the representative in the domain also makes it likely that adversaries will be named. The full equation produces an estimated r-square of .27 and correctly predicts 77.5% of the cases. That seems a reasonably robust result and one in which most of the predictive power is found in the categories of organizations rather than in individual-level variables or in the domain. As we examine the interconnections among these organizational types, we will thus have good reason to regard them as meaningful, apart from other considerations.

When we turn to the actual nominations, we find a very sizable array of organizations named. Table 2 presents the total numbers for each domain. We really cannot say whether the domain differences in Table 2 are large or small because we lack any comparative standard, but it is clear from the ratio of nominators to nominee organizations that in the energy domain the players agree somewhat more fully about both friends and enemies. In the labor area, the adversary list is relatively concentrated, while in both agriculture and health allies and adversaries are more diffuse.

In order to comprehend the specific alliances and antagonisms in each domain, we have sorted both the nominating organizations and their nominees into groups of similar organizational types.

Table 3. Allies and Adversaries in Agriculture

Nominee Organization	Nominating Organizations: Farm Peaks	Commodity Groups	Trade Associations	Externality Groups
Ally				
Farm peaks	30	26	9	6
Commodity groups	27	60	13	5
Trade associations	8	10	41	4
Externality groups	5	0	6	38
Number of respondents	26	35	27	24
Adversary				
Farm peaks	20	18	3	9
Commodity groups	12	3	2	5
Trade associations	2	6	1	15
Externality groups	6	25	41	9
Number of respondents	22	28	19	20

We wished to make the process as parsimonious as possible, but we also felt it necessary to adjust our categories somewhat to take account of elements specific to each policy domain. Many organizations defy easy categorization. Energy firms are often involved in the production or sale of more than one kind of fuel, for example, and a classification of organizations based on fuel type, of critical importance in energy politics (Chubb 1983), encounters difficulties. In agriculture some commodity groups are composed entirely of farmer-producers while others include processors, shippers, commodity brokers, and so on (see Guither 1980). We do not claim to have escaped error, but we believe that most of the more than one thousand organizations named were sensibly classified, and that the findings, accordingly, are credible.

The agriculture domain presents the simplest array of categories, though not the plainest structure (cf. Bonnen 1980; Browne 1986; Hanson 1985). The four categories in Table 3 are *farm peak organizations,* the general organizations of farmers and their subsidiaries; *commodity groups,* producers or producer-dominated organizations specializing in a single crop or farm commodity; *trade associations,* organizations of corporations not directly involved in agricultural production; and, borrowing Hadwiger's (1982) term, *externality groups,* including environmental, welfare, labor, and consumer groups concerned about the externalities of farm policies.

Table 3 presents the number of nominations received by organizations in each category (omitting the considerable number scattered across other kinds of groups). It shows several things of importance. First, the groups in each category tend very strongly to find their allies within their own organizational category. As we will see, this is true also in every other domain. Secondly, farm-peak organizations are internally divided. Both friends and enemies are found there, by the peak organizations and by each of the other sectors. To a lesser extent this is true also of the externality set. Neither commodity groups nor trade associations, on the other hand, often find adversaries internally or in the other. Despite the great diversity within each category and the real potential for conflict between, say, feed producers and livestock interests, the reported organizational alignments do not

Table 4. Allies and Adversaries in Energy

Nominee Organization	Business Peaks	Oil and Gas	Nuclear and Electric	Trade Associations	Environmental Groups
	Nominating Organizations				
Ally					
Business peaks	3	10	1	15	3
Oil and gas	11	57	4	23	0
Nuclear and electric	4	3	38	11	0
Trade associations	6	7	5	24	0
Environmental groups	0	1	8	1	18
Number of respondents	9	32	27	31	8
Adversary					
Business peaks	0	0	0	0	0
Oil and gas	1	14	0	5	4
Nuclear and electric	0	1	5	1	5
Trade associations	0	2	3	1	4
Environmental groups	14	37	51	46	3
Number of respondents	8	27	26	29	8

suggest such conflict. The primary object of adversarial concern among these two sets of specialized interests, commodity groups and trade associations, is the externality category. Apart from the externality groups themselves, few organizations choose them as allies, and they loom very large as opponents. As we will see, a comparable tendency can be observed in other policy domains as well.

The energy field reflects a pattern much like that of agriculture, with one principal exception. In energy politics the role of peak associations—those that transcend particular industries, like the chamber of commerce or the National Association of Manufacturers (NAM)—is quite modest. Here again, however, we see specialized-producer interests working closely with one another and with relatively few adversaries within the industry. Trade associations are less differentiated from producers in energy than they are from those in agriculture, though in both domains they seem to avoid making enemies among producer groups. Hostility from the producer interests and peak business organizations is visited on the environmentalists. Three-fourths of the adversaries named by the energy groups included in Table 4 were environmental groups. Rarely were they said to be allies. Thus they occupy the same functional position in energy as externality groups in agriculture, a category that in that domain also includes environmentalists.

The health-policy domain presents some features resembling energy and agriculture. Here, too, we find that interests select their allies primarily from within their own organizational categories. The externality groups—citizens organizations of various sorts including the Nader-related groups, and labor unions—are often the targets of adversarial relations. More often than in other domains we have examined, however, these groups are also chosen as allies. We take this to mean that the health domain is somewhat less rancorous than others. We noted earlier that a smaller proportion of health respondents had indicated that they regularly encountered opposing groups. Moreover, health representatives perceived their issues to be less conflict ridden and less partisan than did participants in other domains. The diversity of relationships with the *citizen and labor*

Table 5. Allies and Adversaries in Health

Nominee Organization	Nominating Organizations: Medical Associations	Academic Groups	Hospitals	Disease Groups and Nonfederal Officials	Trade Associations	Citizens and Labor
Ally						
Medical associations	41	10	6	3	0	6
Academic groups	15	39	8	0	0	0
Hospitals	14	10	42	3	2	4
Disease groups and nonfederal officials	6	1	0	29	0	10
Trade associations	1	0	0	0	46	1
Citizens and labor	5	5	6	8	6	50
Number of respondents	31	25	23	19	20	25
Adversary						
Medical associations	19	1	0	0	0	7
Academic groups	1	0	0	0	0	0
Hospitals	3	2	1	5	0	11
Disease groups and nonfederal officials	0	0	0	3	2	0
Trade associations	2	1	2	4	3	8
Citizens and labor	17	9	11	6	21	16
Number of respondents	21	13	14	10	16	19

category is further evidence on the same point.

In the health domain there are no genuine peak associations. The American Medical Association might be so regarded, but only for doctors, and it is obvious that doctors are only one among several sets of interest-group participants, albeit a highly significant set. No health group successfully transcends the several major categories of interested parties and lays a persuasive claim to speak for the health-policy community. It may well be that this very fact helps account for the somewhat lower level of acrimony characterizing health politics. Indeed, in an earlier era, when the AMA did largely dominate the full agenda of health issues, the domain seems to have been considerably more conflict ridden (Starr 1982). Analogies to superpowers in international politics may be farfetched, but it is not implausible to argue that the existence of superpowers tends to polarize conflict and that polarization tends to lead to intensification.

Other points of interest in Table 5 include the fact that academic and hospital groups are especially reluctant to name any adversaries. Veterans, a group category not included in the table, carry this strategy to perfection; they claim to have no organized group opponents at all. The congressional politics of health issues is often strenuously contentious, of course, but what these data indicate is that the conflict is not so much among competing interests represented by adversary organizations as with the more amorphous forces of budget constraints and administration priorities. Group politics is an important, sometimes even decisive, part of the overall policy-making process; but it is seldom, if ever, all one needs to know to explain policy outcomes.

We turn now to the labor-policy do-

Table 6. Allies and Adversaries in Labor

Nominee Organization	Nominating Organizations: Labor Peaks	Unions	Citizens Groups	Trade Associations	Business Peaks
Ally					
Labor peaks	2	32	8	2	0
Unions	9	37	11	11	2
Citizens groups	15	17	30	3	4
Trade associations	0	1	5	54	15
Business peaks	0	0	5	30	42
Number of respondents	10	35	26	43	23
Adversary					
Labor peaks	0	0	6	20	22
Unions	1	3	3	20	12
Citizens groups	4	24	24	2	4
Trade associations	1	11	5	7	0
Business peaks	15	41	17	4	2
Number of respondents	10	33	24	33	21

main. Here it is plain that we encounter a bipolar conflict structure. The peak associations of labor and business dominate the poles, and there is almost no trafficking with the other side. Individual unions and trade associations are slightly less implacable foes. The latter, especially, have worked out some alliances with the unions. Citizens groups in this domain are a diverse lot, primarily aligned with labor but including a significant number of antilabor groups, producing substantial intracategory conflict. One revealing feature of Table 6 is that in the conflict-charged climate of labor policy, unlike the other three domains, nearly every respondent was willing to name adversaries as well as allies.

These findings confirm our data regarding the characteristics of issues, where the labor domain evidenced the highest levels of conflict and partisanship. The ally-adversary structure mapped in Table 6 is congruent with the perceptions of labor issues. It is also confirmed by the patterns of event-specific activity reported by our respondents. Groups' relationships to one another in respect to what positions, if any, they had taken on particular policy issues revealed a clear bipolar structure on labor issues. In the other domains, the event-triggered patterns were segmented along lines essentially similar to the organizational groupings identified in Tables 3-6. (For further discussion of these events-based data see Laumann et al. 1986.)

Conclusions

We began by suggesting that the interest-group universe could be regarded as a system of structured conflict (and cooperation). Our data suggest that this is certainly valid at the subsystem, or policy-domain, level (though we are really not able as yet to portray or characterize in any detail the complete "pressure system," to employ Schattschneider's [1960] convenient, if misleading, term). Domain subsystems have been seen to have relatively stable patterns of interaction, quite sharply ideological and bipolar in the labor-policy domain but fragmented, primarily among sets of specialized producer organizations, in the other areas.

Specialized producers, with relatively narrow policy agendas, tend to avoid becoming embroiled in adversarial encounters. As becomes the protagonists in a system of distributive politics, they try instead to confine their efforts to building whatever support they can for their primary policy goals. These live-and-let-live efforts are complicated in some domains by the presence of peak associations, organizations that seek to transcend the limited membership potential of specialized groups like trade associations and to formulate broader policy agendas as they try to speak persuasively on behalf of an entire economic sector. Peak associations tend to express their policy goals in the more encompassing language of doctrine and ideology, using abstraction to rise above the tangible differences of interest among more specialized producers.

The more prominent and unified the peak associations in a policy domain, the more polarized its group structure and policy struggles become. The limiting case in our data is labor, where the peak associations of business and labor do battle and pull the more specialized groups into one orbit or the other. In agriculture, by contrast, the peak associations, once the basis of cohesive bloc politics, are themselves divided. Farm policy-making tends now to be dominated by narrower and often shifting coalitions and to be articulated with less partisan or doctrinal fervor than was the case, say, in the 1950s.

In each of these policy domains, the role of externality groups—environmentalists, consumers, and other "citizens" aggregations—is a kind of mirror image of the peak associations. Where, as in labor, the latter are powerful, the externality groups are diverse, divided, and often serve as satellites to the "superpowers." Where peak associations are divided or nonexistent, the externality interests provide the principal focus of opposition, perceived as getting in the way of the realization of specialized producer interests (see also Schlozman and Tierney 1985, 285-86). Externality groups often like to think of themselves as the guardians of the "public interest" against the diverse claims of multifarious "special interests." With or without that normative language, we find that this structure does indeed characterize three of our four policy domains.

Our interpretation of the role of peak associations in structuring interest-group conflict moves in a very different direction from the principal lines of argument regarding societal corporatism and from the widely discussed contention of Mancur Olson (1982) regarding the significance of encompassing groups. In the corporatist model, effective peak associations of labor and business are necessary elements along with the state in a tripartite structure of cooperative negotiation (Schmitter and Lehmbruch 1979). The singularity of the United States in failing to develop along these lines has been examined elsewhere (Salisbury 1979; Wilson 1982). Our data suggest that even when we do find reasonably effective peak associations, group relations are more rancorous, not more cooperative.

Olson's argument also would lead one to expect that the more encompassing the group (i.e., the more effective the peak associations), the more efficient (i.e., less dominated by narrowly focused producer interests) the policy decisions. The finding that intense conflict develops when the group politics moves from a fragmented, "special" interest-dominated pattern to one in which more broadly encompassing groups are the main players, does not seem compatible with most notions of efficiency. There may, of course, be a stage beyond bipolar group politics in which a single encompassing "group" is able to accomplish fully Pareto-optimal results, but at that point it would appear that we had passed beyond the sphere of democratic politics.

We believe that our findings are relevant to the ongoing debate concerning the validity of pluralist models of U.S. politics. It is obvious that our design has been premised on the assumption that a multiplicity of groups compete in meaningful ways for the rewards at stake in public decisions. But the patterns of group action we have reported go well beyond that. In his classic study of New Haven, Robert Dahl (1961) rests much of his pluralist interpretation on the discovery that business interests (and other interests also) pursued agendas that stopped far short of the full range of public issues and policy decisions. Consequently, their resources, though imposing, were often not brought into play. In his more recent statements, characterized by a considerable degree of recantation, Dahl (1985) and his colleague Lindblom (1977, 1983) place less emphasis on the theme of selective participation as the crucial factor limiting power and protecting pluralism. They stress instead the structural distinctiveness of business interests and the inherent political advantage derived therefrom.

Our data probably cannot illuminate the debate regarding the more abstract properties of business power in a capitalist order. They do show, however, that there is great variety in the forms of politically active business interests (as well as of labor, farmers, professional groups, and others), and that the variety of form is systematically related to the agendas and strategies of action. Trade associations and specialized producers are not simply smaller, narrower segments of a larger class or sector. They often have different goals and operate in different ways, not as anomalies or deviations from some class-defined normalcy but through rational strategy calculated to advance genuine interests.

If, as we have found, the substantive policy domain is a dominant factor in determining the way interest representatives perceive the political context in which they operate and if, further, the structures of domain conflict and cooperation are built from the particular configurations of interest organizations that participate in domain politics, it follows that an adequate understanding not only of interest-group politics but of the full policy process requires us to keep a careful descriptive watch on the group composition of a domain. Unless we know who the organizational players are, how they relate to one another, and what dynamics of organizational development and change are at work, we are unlikely to secure an adequate analytical grasp of the situation. Neither research monographs nor beginning texts should continue to talk about "the farm bloc," the dominant role of the American Medical Association, or the unity of "business" interests when events have long since rendered those empirically invalid. Our effort to describe the structures of conflict and cooperation among organized groups may thus be taken as a contribution to an ongoing obligation to attend systematically to the *substance* of interest-group politics.

In the end, of course, we must be concerned not only with the structures of interest-group relations but with their policy outcomes as well. What is the reflection in substantive decisions of the fragmented politics of agriculture or the bipolar conflicts over labor issues? Does a "play-it-safe" trade association accomplish more of its objectives than a contentious citizens group? In the next phases of our research, we hope to be able to speak to these questions. Meanwhile, however, showing that interest-group interactions display patterns interpretable in their own terms and comparable in structure to those in other realms of political analysis provides an avenue of potential linkage to the broader bodies of theory on which a coherent discipline of political science must rest.

Appendix: Methods of Sampling Client Organizations

We arrived at our sample of client organizations by drawing on four sources of nomination that could measure levels of policy-making activity (Shapiro 1984). Our first method for locating interested actors in a policy domain was especially attentive to issues attracting mass-media attention because of their controversiality and broad popular interest. Such issues are especially likely to appear in the congressional arena. Organizations taking partisan public stands on popular issues are especially likely to be identified by this technique. We conducted a computerized search of stories in national and regional newspapers and magazines dealing with federal policy-making in each domain from January 1977 to June 1982, noting the number of stories mentioning each organization. The data regarding newspaper coverage of organizational participation were compiled from the Information Bank, a data base of the New York Times Information Service (NYTIS). To compensate for regional effects, the data base included the *Chicago Tribune, Houston Chronicle, Los Angeles Times, New York Times, San Francisco Chronicle, Seattle Times, Washington Post,* and *Time* magazine. The product of the computer search was an extensive set of news-story abstracts from the source newspapers. These abstracts were then searched and tagged for the names of organizations, and lists of organizations in each domain were compiled. The number of mentions for each organization was recorded, a mention being defined as an appearance in one newspaper abstract. Duplicate abstracts of stories reported in two or more news sources were eliminated.[1]

Second, we searched the abstracts of congressional hearings held by committees and subcommittees with jurisdiction in each of the four domains during the 95th-97th congresses, noting the number of hearings at which organizations testified. Less publicly partisan and more specialized organizations are revealed by this procedure. In light of the enormous number of hearings covered in the Congressional Information Service (CIS) database, we restricted the search to the first sessions of the 95th, 96th, and 97th congresses and to select major committees active in each domain.

Third, during July 1982, we interviewed 20-23 government officials in various policy-making positions in each domain in the Congress and the executive agencies, noting the number of officials mentioning an organization as one that frequently contacted them or which was representative of organizations that contacted them episodically. This method is especially likely to identify organizations having direct dealings and concerns with particular executive agencies and their regulatory initiatives. Two criteria were used in selecting individuals to be interviewed: (1) the position of the individual in the unit and (2) his or her tenure in the unit. An attempt was made to avoid relying exclusively on politically appointed officials or on those with less than two years' tenure in office.

Finally, for each domain we compiled a list of organizations appearing under the industry headings raised to the domain in *Washington Representatives* (1981), an annual publication that canvases various public sources and surveys organizations in an effort to list organizations represented before the federal government. Organizations that are self-professed lobbyists before Congress and the executive agencies are especially likely to be identified.

Each source method of nominating organizations for inclusion in the population of claimant organizations has distinctive features that sets it apart from the others (Salisbury 1984). If we had relied only on the listing produced by the *Washington Representatives,* we would have underestimated the population of interested organizations by more than half in each domain. (The percentage of organizations *not* contained in the *Washington Representatives* directory was 70.4% in agriculture, 52.8% in energy, 67.1% in health, and 80.3% in labor.) Domains differ substantially among themselves with respect to the amount of newspaper attention given them, the raw numbers of organizations identified as active, and the number of organizations engaged in repeated efforts to influence policy outcomes in their respective domains. The energy domain seems to have the largest number of organizations attracted to its concerns, with agriculture and health roughly tied in scale of participation with almost a third fewer organizational participants, and labor placing a rather distant fourth. While the CIS ratios of the number of mentions to the number of organizations mentioned are remarkably stable across the four domains, the NYTIS ratios vary considerably across domains. There is a high ratio in energy, for example, suggesting the relative prevalence of "repeat players" in the domain.

For each domain we drew a random sample of one hundred organizations, with each of the four sources contributing equally, but with each organization in the first three sources weighted by the number of mentions in that source. Because, for all but the listing from *Washington Representatives,* the probability of selection increased with the number of mentions, our sampling procedure reflected each organization's level of activity in the domain. We had strong reasons to adopt this weighting procedure. Knoke and Laumann (1983) had demonstrated that there was a high correlation (.75 and .72 for the energy and health domains, respectively) between the number of organizations active in a domain that named an organization as among the most influential actors in the domain and the number of mentions in newspaper stories, appearances in congressional hearings, and mentions by government officials. Hence, there was solid evidence for treating the number of mentions in the various sources as a measure of perceived organizational influence. The sampling procedure thus generated a list of organizations to be interviewed that disproportionately included the more influential and active organizations in the domain, while also allowing the selection of less influential and active organizations. Simple inspection of the samples reveals that we were quite successful in including many of the most prominent and influential organizations, with an admixture of less visible, more peripheral organizations. This mixture of organizations will permit us to investigate what, if any, systematic differences are to be observed in modes of participation in policy deliberations across different types of organizational actors.

Having sampled the client organizations, we then identified the representatives they employed or retained by conducting telephone interviews with client informants in a minimum of 75 organizations in each domain, 316 organizations in all.[2] These informants were identified by using published listings of organization officers, supplemented with direct inquiries of the organizations to determine who had operating responsibility for the organization's involvement in federal policy in the appropriate domain. The informant was asked to name up to four individuals inside the organization (employees, officers, member volunteers) and up to four individuals external to the organizations (employed in outside firms, trade associations, and so forth but not falling within the definition of an internal representative) who acted as key representatives for the organization in the policy area. It is this listing of nominated individuals that constituted the population of representatives.

The client interviews generated between 400 and 450 names of representatives in each domain, for a total of 1,716 individuals. (Representatives could appear on the lists as many times as they were mentioned; about 5% appeared in more than one domain.) Random selection from these lists produced samples of 257-61 representatives per domain, with a realized sample ranging from 192 to 216 across the four domains. The overall response rate of representatives was 77%. About 10% of the representatives contacted declined interviews; the rest could not be scheduled for various reasons. Slightly over two-thirds of the respondents were based in Washington and were personally interviewed there. Another 15%, based in other major cities, also were interviewed personally. The remainder of the sample was interviewed by telephone using an adapted format.

The final set of actors to be identified were the government officials most often the targets of representational activity in a given domain. These target government officials (including those holding elected, appointed, and career positions) were identified by asking the representatives we sampled to give the names and positions of "the five government officials or staff members you contact most often in the course of your work" in the appropriate policy domain. From these lists we sampled from 101 to 108 names in each domain, and successfully interviewed about 72%. About 8% of the government officials declined to be interviewed.

Notes

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1. For a presentation of the specific data see Nelson et al. 1987.

2. The response rate was 78% with 10% of the organizations refusing interviews. Another 12% of the organizations could not be located, were located overseas, or had ceased to exist. Refusals did not follow any discernible pattern and were not regarded as a significant source of bias.

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NATIONALIZATION AND PARTISAN REALIGNMENT IN CONGRESSIONAL ELECTIONS

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The relationship between nationalization of the U.S. electorate and partisan realignment is explored. Concepts and measurement of nationalization are examined, as well as definitions of electoral change. The British concept of swing is utilized as an appropriate measure of electoral change. Examination of long-term trends in the variances of the congressional vote and swing from 1842 to 1980 shows they are related to the electoral dynamics of realignment. Analysis utilizing a variance-components model shows there has never been a nationalization in terms of configuration of the electorate. But nationalization in the movement of the electorate has taken place cyclically, corresponding to the partisan realignments of the 1890s and the 1930s, rather than monotonically, as suggested by previous research.

One of the major problems of U.S. political science has been to identify the various forces working to shape partisan votes in the electorate and to chronicle their change over time. Among the most important efforts has been Donald Stokes's model, which partitioned the variance of the congressional vote into national, state, and district components (Stokes 1965). Stokes found that since the Civil War era there has been a growing nationalization of the electorate in the United States (Stokes 1967). He attributed this nationalization to the changing structure of mass communication.

During the two decades since its appearance, only two works have directly assessed the adequacy of Stokes's method (Claggett, Flanigan, and Zingale 1984; Katz 1973). Claggett, Flanigan and Zingale questioned the validity of Stokes's conclusion by showing that the estimated "national component," which Stokes did not present in his paper, has not grown for a century (1984, 79, n. 1 and Fig. 2). Reanalyzing the question, they claimed to use "basically the same methodological approach to the problem" as Stokes (p. 84). However, they changed handling procedures originally set by Stokes, using county-level election returns instead of district-level data. They used regions instead of states as an intermediary aggregate level and extended the period of analysis to include the decades from the 1840s to the 1960s. In this reanalysis they found no evidence of growing nationalization.

That Claggett, Flanigan, and Zingale reached different conclusions from Stokes is intriguing because they felt sure their measure of the national effect was "strictly comparable to the 'national effect' in Stokes's analysis" (p. 84). Yet the results associated with the second measure in their paper are quite different from those associated with the first. (Compare

Stokes's estimated national effect with their national effect in Claggett, Flanigan, and Zingale 1984, Figs. 2, 4.) Claggett, Flanigan, and Zingale did not refer to this discrepancy at any point. In light of such discrepancies, their statement that the "differing conclusions rest on examination of additional and more appropriate data, not on any variations in method that may have been introduced" (p. 89), is not persuasive. It seems that Claggett, Flanigan, and Zingale cast serious doubt on the validity not only of Stokes's argument but of their own as well, for valid methodology applied to similar data should produce similar results.

Not only has some doubt been cast on the reliability of Stokes's conclusions, but his findings do not mesh very well with other literature on the U.S. party systems (see Niemi and Weisberg 1976 for a similar observation). There were two partisan realignments in the period Stokes analyzed: the realignment following the election of 1896 and the realignment of the New Deal era. Yet these political cataclysms do not seem to have affected the constant process of nationalization that Stokes identified. A natural question is, What has been the relationship between the degree of nationalization of the U.S. electorate and partisan realignments?

Electoral analysis of realignment has focused mainly on presidential elections and has tried to identify the breakpoints in partisan support in a series of elections (Key 1955). Therefore, the national partisan vote has been the center of attention. This approach has led to the refinement of critical realignment theory by identifying distinct stages in party systems (for example, see the t-score analysis in Burnham 1970). But there are some problems. To focus on the national partisan vote deflects our attention from the diverse nature of U.S. politics. The national partisan vote is a summary measure of votes cast in various districts within states. Therefore the national "cutting points" marking transitions between party systems were the outcomes of complicated processes at the subnational levels (see, for example, a study of realignment in New York state in Benson, Silbey, and Field 1978). The nationalization approach Stokes initiated can be useful in analyzing this aspect of realignment.

A recent article by David Brady (1985) suggests one clue for the application of the nationalization approach to the analysis of realignment. In an effort to redirect our attention to a policy-oriented approach to realignment, Brady has proposed a theory of policy change. One of his propositions is of particular interest here because it posits a link between nationalization and realignment. In periods of realignment, he has written, "the election results were more national, less local" (p. 30). In order to test this proposition, he checked the coefficients of variation of the aggregate congressional-vote swings. He found that coefficients of variation tended to be smaller during a realignment period than at other times, indicating that across-the-board movement of the electorate was more prevalent at such times. This analysis builds upon Brady's insight and approach, showing that the degree of nationalization in elections is related to the dynamics of realignment, and presenting a model appropriate to analyzing this relationship.

Concepts of Nationalization

Previous research on the nationalization of the electorate has sometimes been ambiguous conceptually or methodologically about exactly what has been nationalized. For example, Stokes (1965, 1967) was concerned with the political levels at which forces moving the electorate arise. Stokes (1965) initially partitioned the total variance of the *change of voting* into between-state and within-state (district-level) variances (Stokes 1965, 63–65). Yet when he extended his model to incorporate the national term, he replaced the *change of voting* with the *vote itself*, over a series of elections. Stokes argued as if the transition from the first model to the second were a simple extension of his methodology. Yet it was in fact a transformation of his model and perspectives, for the two variables Stokes employed in the two models are conceptually distinct.

Claggett, Flanigan, and Zingale (1984) clarified the two concepts of nationalization. They identified one type of nationalization as convergence in the levels of partisan support. This concept is related to the partisan vote at each district. If reaction to the national party plays the most important role in an election and the composition of the electorate in most districts is similar, then it is probable that every district will show a similar level of partisan support. The fortune of a party candidate in one district will be closely connected to that of the party's other candidates across the country. Nationalization as convergence refers to the homogeneity of the electorate. I will use the term *configuration* to indicate this attribute of the electorate.

The electorate with a nationalized configuration is one that shows few regional and district differences in partisan support. But one of the important characteristics of U.S. electoral politics is that local factors have always played a significant role. It cannot be hypothesized that the U.S. electorate was truly nationalized at any time in history. The traditional existence of regional difference in partisan support, clearly seen in presidential elections, militates against the existence of a nationalized electorate (Burnham 1981a, 1981b). Moreover, the decline of competition during the last 20 years in districts where congressional incumbents are seeking reelection suggests that there has been, if anything, *de*nationalization in recent decades (Mayhew 1974).

The second concept of nationalization is that of a uniform response to political forces. This concept considers the *movement* of the electorate as distinguished from its basic configuration. Claggett, Flanigan, and Zingale described this aspect of nationalization as a "situation in which all subunits are responding with a uniform surge toward (or away from) a particular party" (1984, p. 82). Thus districts may show concordant movement even though they are not homogeneous in configuration. If there is the same direction and amount of electoral change in every district, then the electorate is perfectly nationalized in movement. Stokes was only concerned with nationalization as a uniform political response, not as a uniform configuration.

Measurement of Electoral Change

In order to assess the degree to which the nation's vote is nationalized in configuration, I will analyze how the partisan vote is dispersed at different levels of aggregation. As to nationalization in the movement of the electorate, one must turn to some indexes of electoral change.

One of the influential definitions of aggregate electoral change in the U.S. electoral-research literature is that of Flanigan and Zingale (1974).[1] They elaborated on types of electoral change, paying attention to the uniformity and the persistence of change. But their basic idea was to define *aggregate electoral change* as "movement away from normal voting patterns" (p. 56). The normal voting pattern was operationalized as the grand mean of the array of elections. It was assumed that "the fluctuations in the actual vote over a period average out to an approximation of the underlying partisanship of the electorate" (Clubb, Flanigan, and Zingale 1980, 89). The vote was considered to move away from the underlying grand mean at each election.

Defining *change* as movement away from the normal voting patterns creates some difficulties. First, the normal vote pattern should be estimated somehow, which is hard to operationalize in electorally turbulent years. The second difficulty arises with the use of this electoral change in the analysis of variance techniques; that is, if the amount of electoral change in one year is measured by the deviation of the vote from its over-time mean, then it follows that the amount of change in the entire set of elections analyzed may be measured by the over-time variation of the vote (see Flanigan and Zingale 1974, 76–77, n. 5). Thus, Claggett, Flanigan, and Zingale (1984) employed the over-time variation of the national mean (normalized election sum of squares) to measure national effect as a uniform response. Stokes's (1965) national component is basically the over-time variance of the national mean vote. I do not think this is an appropriate way to measure electoral change.

Let me illustrate the problem by an example, where a series of elections yields 40%, 40%, 40%, 50%, and 55% for a certain party. The support for this party is the same for the first three elections, and then it increases monotonically. The over-time mean of the vote is 45%. The amount of change in the period is measured by the over-time variation of the vote, that is, 3.5 according to Claggett, Flanigan, and Zingale's method. Consider another series of elections where one party gets 40%, 50%, 40%, 55%, and 40%. The support for this party is quite unstable in the period. A significant portion of the electorate changes partisan allegiance at each election. However, this set of elections has the same amount of over-time variation as that of the previous one, for only the sequence of the values of the vote is different between the two sets. The above examples clearly show that the sequence of elections does not affect the over-time variation of the vote. But *change* is a time-related concept. I think that the sequence of elections should be taken into account when measuring change.

I suggest defining *change* as the difference between two electoral results. My preference is grounded both on the simplicity of the definition and the similarity of this definition to the British concept of *swing*. British scholars define *swing* as the average of the Conservative percentage gain and the Labour percentage loss (Butler 1951; see also Steed 1965). If there is no strong third party to be counted, then *swing* will simply be the Conservative gain, for the gain of the one party is the loss of the other. Thus *swing* is equivalent to my definition of electoral change for one party when there is no third party. *Swing* captures net aggregate movement of the vote from one election to a second one. Stokes (1967) and Brady (1985) also utilized swing in their analyses. Hereafter, I will use the expressions *my measure of electoral change* and *swing* interchangeably because of their similarity. They mean the partisan vote percentage in each district in one election minus the partisan vote percentage in the previous election.

By using swing, one can measure the amount of change for the period in the above examples more appropriately. Two points should be made clear beforehand. First, variance is a measure of dispersion. It measures the degree to which values differ from each other. Thus, the over-time variance of the vote does not measure the amount of change. It rather measures the *configuration* of the vote within the period analyzed, that is, how each election is distinct from others in the period. Second, in order to capture the electoral change for the period, I will look at both the mean and the variance of swing over time. The over-time variance of swing measures the dispersion of swing in the period analyzed.

For the first series of elections in the above example, the swing is 0%, 0%,

Table 1. Variance of the Democratic Vote across Districts

Year	Variance	Number of Cases	Year	Variance	Number of Cases	Year	Variance	Number of Cases
1842	94.4	181	1888	155.1	304	1934	204.1	339
1844	94.8	147	1890	205.4	314	1936	281.7	373
1846	121.0	180	1892	157.8	329	1938	260.9	341
1848	242.9	201	1894	223.2	334	1940	271.1	351
1850	127.8	175	1896	210.5	257	1942	227.7	327
1852	185.6	206	1898	281.8	294	1944	280.8	367
1854	161.1	153	1900	286.5	317	1946	240.9	354
1856	218.0	209	1902	315.9	327	1948	238.0	353
1858	212.6	221	1904	367.1	342	1950	213.4	340
1860	118.4	145	1906	345.0	324	1952	194.5	343
1862	159.3	163	1908	265.5	348	1954	194.9	350
1864	96.4	164	1910	331.6	327	1956	180.5	358
1866	150.7	172	1912	329.8	353	1958	204.9	344
1868	163.0	217	1914	385.0	351	1960	191.0	360
1870	144.6	225	1916	309.4	347	1962	211.6	380
1872	136.6	269	1918	239.0	308	1964	199.2	394
1874	139.0	242	1920	375.3	364	1966	257.2	386
1876	135.5	285	1922	361.9	357	1968	262.4	390
1878	291.9	271	1924	412.3	355	1970	303.5	386
1880	152.1	278	1926	431.6	338	1972	302.8	389
1882	189.0	292	1928	274.2	360	1974	278.4	382
1884	133.2	298	1930	350.3	328	1976	315.1	401
1886	204.8	286	1932	287.4	344	1978	337.0	359
						1980	380.1	369

Note: Districts where Democratic candidates earned more than 98% or less than 2% of the total vote were excluded from the analysis.

10%, and 5%. The over-time mean swing is 3.75%, and the variance of swing is 17.2. The second series has greater swings: 10%, −10%, 15%, and −15%. The over-time mean swing is 0% and the variance is 162.5. The over-time mean indicates that there is an increase in the party support in the first series but not in the second. But the variance of swing indicates there is more change in the second than in the first.

Variance of the Vote across Districts and Variance over the Decade

The first step in my strategy for analyzing configuration and movement in U.S. congressional elections is to show the variances of the Democratic vote and swing. Table 1 shows the variance of the Democratic vote across districts in each congressional election since 1842. Every contested result available was used for the calculation.[2] The variance shows the degree to which there was diversity in partisan support. A small amount of variance means that the nation was relatively homogeneous in the level of support the Democratic party received. The larger the variance, the more diverse the level of partisan support in the districts.

There are two types of change in the variance. First, there is short-term fluctuation lasting only several years. For example, the amounts of variance went up and down slightly through the 1940s. One reason for this fluctuation was the change in the number of contested districts. When there have been many contested districts, variance has tended to be large; smaller variance has often been associated with a smaller number of contested districts. Generally speaking, midterm elections tend to have smaller numbers of contested districts because congressional aspirants do not expect to benefit from the coattail effects of presidential candidates. Thus the decisions of congressional aspirants seem to affect the amount of variance.

Second, there are long-term trends in the amount of variance. Table 1 shows that variance decreased around the Civil War and then increased, reaching its highest point in the 1920s. Variance declined in the 1930s for the first time since the Civil War. It hit bottom in the 1950s and again increased over the last 20 years.

One might posit that the long-term trend can be explained by changes in the number of contested districts too. For example, the growth in the amount of variance from the 1860s to the 1920s might have been caused by the growth of the nation and the consequent increase in the number of contested districts. But the trend after the 1930s militates against this explanation, for the decrease in the amount of variance did not accompany a decrease in the number of contested districts.

Thus the long-term trend probably really reflects changing diversity in Democratic support across districts and states. There was a trend toward greater variance through the Reconstruction era, and the realignment of the 1890s accelerated it. The regional cleavage that ran across the nation at that time was between the northeast metropolitan industrial area and the South and West. The Northeast was growing progressively more Republican, while the South and West were becoming more Democratic, producing ever higher variance. One should expect the realignment of the 1930s to have had a strong impact on the configuration of the electorate, because to some degree U.S. citizens divided politically along class lines. This greater class polarization weakened regional bases of political parties, producing smaller variance.

The trend from the 1950s to the present needs special attention, for congressional elections have become increasingly insulated from presidential elections (Burnham 1975b; Mayhew 1974). In his analysis of presidential elections, Kleppner has shown that the variance across regions of Democratic longitudinal means for 1894–1930 was 136.08, while that for 1932–72 was 39.93 (Kleppner 1981, 19, Tbl. 1.2). Moreover, the variance for 1932–60 was 73.59, while variance for 1960–72 was only 10.73. Judging by Kleppner's analysis, presidential elections have become increasingly nationalized in recent decades. In light of this, the growing variance in congressional elections over the past 20 years indicates an insulation of congressional votes from the national political scene.

To further analyze the degree of nationalization in elections over time, I have calculated *decadal variance* to show the configuration in each decade. Decadal variance is calculated by using the partisan vote in each district over five consecutive elections after redistricting. It measures the degree to which the vote is dispersed from the grand mean of the whole decade. This differs from the variance across districts just displayed. That variation is the deviation of the district vote from the national mean in each election year. In decadal variance, the variation stems both from the deviation of the national mean in each election year from the grand mean of the decade and from the deviation of the district vote from the national mean. Thus by using decadal variance I bring the national variation into consideration of configuration. But although it is possible to distinguish national variation from subnational variation conceptually, it is not so easy to operationalize, since the national partisan vote is the outcome of

Table 2. Indicators of the Democratic Vote and Swing

Decade	Decadal Variance of the Vote	Range of Variance of Swing	Mean of Variance of Swing	Decadal Variance of Swing
1840s	141.7	156.5	134.2	151.1
1850s	185.8	27.9	115.7	127.9
1860s	147.8	106.5	64.1	74.4
1870s	179.1	110.4	124.2	141.2
1880s	180.3	51.4	98.5	101.3
1890s	242.5	14.6	61.6	87.7
1902–10	326.5	27.6	56.2	67.6
1912–20	335.3	29.0	73.6	87.4
1920s	370.9	52.4	77.8	89.9
1930s	271.1	24.6	46.7	53.4
1940s	250.1	40.4	46.1	67.6
1950s	199.7	15.9	33.8	50.6
1960s	252.1	21.0	65.4	85.0
1970s	326.5	69.2	123.6	137.3

various votes counted at district level. It is simply an aggregated value, and it reflects more than a uniform or "national" element. Thus, variance of the national mean from the grand mean of the decade reflects both the national variation and the subnational variation. In order to measure the amount of national variation, one has to remove the part that reflects the subnational variation.

In Table 2 the decadal variance of the Democratic vote since the 1840s is shown. One can find almost the same secular trend as that shown in Table 1: the increase in variance from the 1860s till the 1920s, followed by the decrease for the next three decades and then the recent increase.

Variance of the Swing across Districts and Variance over the Decade

Brady (1985) calculated the mean swing and the variance of swing to analyze electoral change in prerealignment, realignment, and postrealignment periods. Brady's choice of elections for the calculation of swing seems arbitrary, in that different time periods went into the calculation. In order to avoid this arbitrariness, I decided to use a fixed time period of two years for calculating variance of my measure of electoral change, in the manner of Stokes (1967), and I expanded his analysis to include 1842–1980.

Table 3 shows variance of the Democratic swing across districts from 1844. The values for the first years after each reapportionment are not shown because the change in district boundaries makes the calculation unreliable. Every contested pair of district election results was utilized for the calculation. This procedure is almost the same as the one Stokes (1967) used in his analysis, and I obtained similar results to his for the 1950s.

A pattern is not so easily discernible as it was with variance of the vote itself. But again there are two types of change. One is the short-term fluctuation over a few years' time. The movement of the electorate was in no way uniform across districts, and the degree to which local swing diverged from the national mean swing fluctuated. This fluctuation was the result of various local responses that sometimes coincided by chance to produce a rather uniform response and thus small variance. But these usually did not amount to a nationally uniform movement toward or away from one party. Examples of this type of fluctuation are abundant in Table 3 where no two adjacent figures are the same.

The second change involved longer time periods. Variances of the swing in one decade fall within some range, and thus the decades show certain levels in the size of variances. From Table 3, I calculated the *range* of values in each decade (see Table 2). In the 1840s, the 1860s, and the 1870s the range was larger than 100. But in the rest of the decades, the range was rarely larger than 50. Interestingly, one finds smaller range in the 1850s, the 1890s, the 1930s, the 1950s, and the 1960s than in other decades.

Then I calculated a *mean* of the variances of the swing over each decade (see Table 2). The mean hit bottom three times in 140 years. The first was in the 1860s, just after the realignment of the 1850s. The second time was in 1902–10, just after the realignment of 1896. The third was in the period from the 1930s through

Table 3. Variance of the Democratic Swing across Districts

Period	Mean Swing	Variance	Number of Cases	Period	Mean Swing	Variance	Number of Cases
1842–44	.4	61.3	125	1912–14	- .9	78.5	314
1844–46	-3.0	64.2	129	1914–16	1.4	62.2	312
1846–48	-2.7	193.4	151	1916–18	- .5	62.4	266
1848–50	5.0	217.8	154	1918–20	-8.4	91.2	288
1852–54	-4.3	127.7	129	1922–24	-5.6	65.8	324
1854–56	5.6	101.0	139	1924–26	2.4	49.5	316
1856–58	.7	128.9	200	1926–28	- .2	93.8	316
1858–60	-1.1	105.2	140	1928–30	3.6	101.9	308
1862–64	-4.2	57.0	141	1932–34	.3	62.6	294
1864–66	.7	45.3	156	1934–36	.9	43.8	332
1866–68	2.7	23.7	171	1936–38	-5.4	38.0	335
1868–70	1.1	130.2	213	1938–40	1.0	42.4	328
1872–74	5.0	115.8	215	1942–44	2.1	73.5	316
1874–76	- .8	67.0	232	1944–46	-4.9	40.7	337
1876–78	-5.7	177.4	256	1946–48	7.2	36.9	329
1878–80	2.2	136.7	251	1948–50	-2.5	33.1	319
1882–84	.1	120.2	269	1952–54	4.4	27.1	323
1884–86	.6	115.0	264	1954–56	-2.1	27.5	335
1886–88	- .8	90.0	267	1956–58	6.8	37.5	328
1888–90	4.1	68.8	287	1958–60	-2.9	43.0	329
1892–94	-7.9	55.4	317	1962–64	4.6	54.4	361
1894–96	1.7	56.7	252	1964–66	-6.6	65.6	360
1896–98	6.1	70.0	240	1966–68	- .5	66.1	361
1898–1900	- .6	64.2	284	1968–70	3.6	75.5	368
1902–04	-5.5	68.3	311	1972–74	6.1	166.2	354
1904–06	3.2	45.2	302	1974–76	-1.2	97.0	361
1906–08	- .1	41.9	317	1976–78	-1.3	124.7	337
1908–10	2.2	69.5	318	1978–80	-3.8	106.5	335

Note: Districts where there was no competition in either of a pair of elections were excluded from analysis for these years.

the 1950s, when the New Deal realignment and its aftershocks spread over the country. Students of U.S. party systems will easily arrive at a hypothesis about the relationship between these changes in variance and the electoral dynamics of realignment.

Next I will turn to the decadal variance, as I did in the analysis of configuration of the vote. As mentioned earlier, decadal variance incorporates national variation and subnational variation. If the decadal variance of swing is small, then it is likely that the movement of partisan support is uniform across districts and over a decade, suggesting the existence of nationalization.

Table 2 shows the decadal variance of swing for the Democratic party. It shows similar trends to range and mean of the variance of swing in each decade. The decadal variance of swing hit bottom in the first decade following a realignment (the 1860s, 1902–10, and the 1930s). And the decadal variance was small in the 1940s and the 1950s. With the reservation that decadal variance incorporates both the deviation of district swing from the national mean in each year and the deviation of the national mean from the grand mean of the decade, findings are neatly summarized by decadal variances.

A Model for the Assessment of Electoral Forces

The previous sections introduced decadal variances of the Democratic vote and swing, indicating that decadal variances are closely connected to the dynamics of electoral realignment. I would like to establish a model for the assessment of electoral forces. As explained above, the decadal variance is the mean of squares of the deviations from the grand mean. The vote and my measure of electoral change upon which decadal variance is based have three levels of aggregation: district, state, and national. It is possible to partition the decadal variance into these components by a nested model of analysis-of-variance technique. By doing so, one may estimate how much of the variance is attributable to national, state, and district components.

My model can be described verbally as follows: *decadal variance = national variance + state variance + district variance. National variance* is the variance of the national mean from the grand mean, while *state variance* is variance of the state mean from the national mean, and *district variance* is variance of the district value from the state mean. But because the model is a nested one, means at each higher level of aggregation must be adjusted so that they do not reflect the variation at lower levels. Thus, one can estimate national means and state means only through the model.

The structure of my model resembles those of Stokes (1965) and Claggett, Flanigan, and Zingale (1984). But there are major differences: While Stokes used the difference between the vote percentage and its mean over the decade in each district in his calculation of decadal variance, I use the vote percentage itself to analyze configuration, and swing to analyze movement (see the Appendix for a further discussion and criticism of Stokes's model). Stokes's data corresponds to what Flanigan and Zingale (1974) called *electoral change,* because it is movement away from the normal voting patterns. For the reasons discussed above, I did not use Flanigan and Zingale's definition of *electoral change* in the analysis. Since the decadal variances of the vote and the swing are consistent with the understanding of realignment and electoral-history analysis, it seems appropriate to utilize them as the data for the nationalization analysis. The results indicate that this approach is fruitful.

A reader who is not interested in the statistical detail may skip the following

formal description of the model and proceed to the next section.[3]

To put the model formally, I first assume that the vote or the swing in the jth district of the ith state in the kth year Y_{ijk} can be described as a sum of four elements:

$$Y_{ijk} = M + A_k + B_{ik} + C_{ijk} \qquad (1)$$

In the equation, M is a constant, which is the grand mean $Y_{\cdot\cdot\cdot}$, and A_k, B_{ik}, and C_{ijk} are national, state, and district effects. It is also assumed that there are J_i districts in the ith state, that I states make up the nation, and that the results of K years are in the analysis. For the sake of explanatory convenience, I assume here that every district is contested in each year analyzed. A_k, B_{ik}, and C_{ijk} are assumed to have a mean of zero. Also, they are assumed to be uncorrelated.

Because the three random variables are independent,

$$\begin{aligned} \mathrm{var}(Y_{ijk}) &= \mathrm{var}(A_k) + \mathrm{var}(B_{ik}) \\ &\quad + \mathrm{var}(C_{ijk}) \end{aligned}$$

The left-hand-side expression is the decadal variance of the vote or the swing. The purpose is to estimate the three variances in the right-hand side of the equation.

By the method of analysis of variance, total SS of Y_{ijk} is partitioned into three SSs in the following way:

$$\begin{aligned} &\Sigma_i\Sigma_j\Sigma_k(Y_{ijk} - Y_{\cdot\cdot\cdot})^2 \\ &\quad = \Sigma_i\Sigma_j\Sigma_k(Y_{\cdot\cdot k} - Y_{\cdot\cdot\cdot})^2 \\ &\quad + \Sigma_i\Sigma_j\Sigma_k(Y_{i\cdot k} - Y_{\cdot\cdot k})^2 \\ &\quad + \Sigma_i\Sigma_j\Sigma_k(Y_{ijk} - Y_{i\cdot k})^2 \qquad (2) \end{aligned}$$

(A dot in the subscript of Y_{ijk} means that Y_{ijk} is to be averaged over the subscript that is replaced by the dot.)

Degrees of freedom associated with the terms in Equation 2 are $(K\Sigma_iJ_i - 1)$, $(K - 1)$, $K(I - 1)$, and $K\Sigma_i(J_i - 1)$, respectively. Thus, the mean squares of the three effects are described as follows:

$$MSA = \Sigma_i\Sigma_j\Sigma_k(Y_{\cdot\cdot k} - Y_{\cdot\cdot\cdot})^2/(K - 1) \qquad (3)$$

$$MSB = \Sigma_i\Sigma_j\Sigma_k(Y_{i\cdot k} - Y_{\cdot\cdot k})^2/K(I - 1) \qquad (4)$$

$$MSC = \Sigma_i\Sigma_j\Sigma_k(Y_{ijk} - Y_{i\cdot k})^2/K\Sigma_i(J_i - 1) \qquad (5)$$

Next, I will express the expectations of the three mean squares by linear combinations of the variances of the random variables. They are obtained by substituting the right-hand-side expressions of Equation 1 for terms in Equations 3–5. For example, $Y_{\cdot\cdot k}$ can be written as

$$\begin{aligned} Y_{\cdot\cdot k} &= \Sigma_i\Sigma_j(M + A_k + B_{ik} \\ &\quad + C_{ijk})/\Sigma_iJ_i \end{aligned}$$

Hence,

$$\begin{aligned} E(MSA) &= \Sigma_iJ_i\,\mathrm{var}(A_k) \\ &\quad + (\Sigma_iJ_i^2/\Sigma_iJ_i)\,\mathrm{var}(B_{ik}) \\ &\quad + \mathrm{var}(C_{ijk}) \qquad (6) \end{aligned}$$

$$\begin{aligned} E(MSB) &= [1/(I - 1)](\Sigma_iJ_i \\ &\quad - \Sigma_iJ_i^2/\Sigma_iJ_i)\,\mathrm{var}(B_{ik}) \\ &\quad + \mathrm{var}(C_{ijk}) \qquad (7) \end{aligned}$$

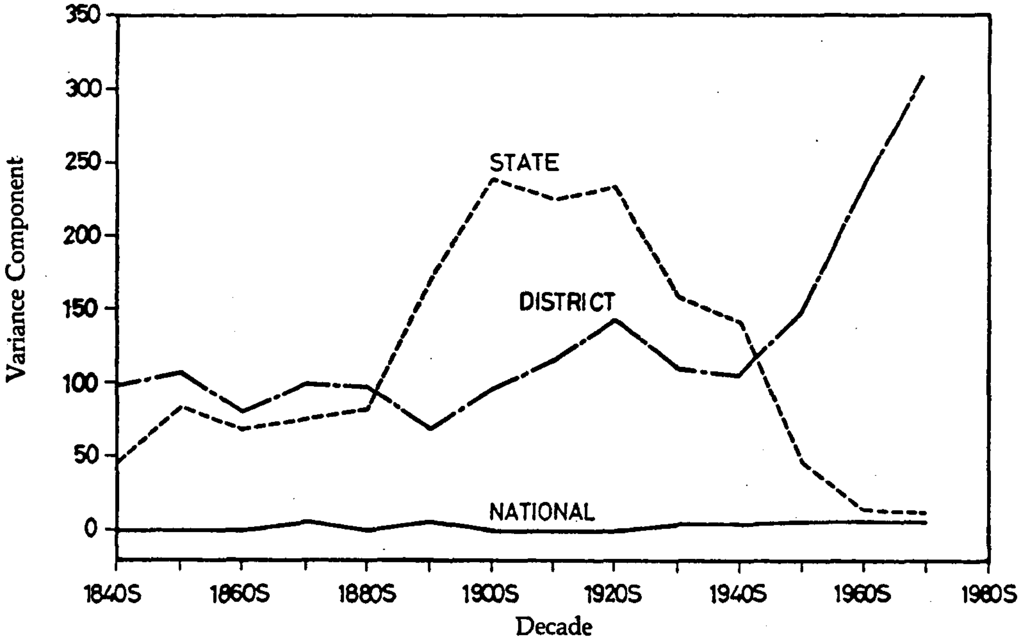
$$E(MSC) = \mathrm{var}(C_{ijk}) \qquad (8)$$

By equating the mean squares obtained by Equations 3–5 to the expected mean squares of Equations 6–8, we get the estimates of the three variances.[4]

Results—Configuration of the Electorate

Figure 1 shows the results of the analysis of the configuration of the Democratic vote from the 1840s to the 1970s. The lines represent the amounts of variance attributable to national, state, and district components. The sum of the three com-

Figure 1. Configuration of the Democratic Vote



ponents is the decadal variance of the vote.

There has never been a nationalization of the U.S. electorate in terms of convergence. The national component explains very little, if any, of the total variance of any decade. This means that the national mean in one year was not much different from the mean in other years, after adjusting for differences at the district and state levels. But at the subnational level, the configuration looks quite different. What emerges here is the changing shape of the U.S. electorate during the past century.

The United States had a fairly homogeneous electorate before the Civil War. In this period the total variance of the vote was evenly divided into state and district components. It seems that the Republican realignment did not much affect the configuration of the electorate. Another analysis was conducted using the data that included uncontested districts, since the effects of a growing number of uncontested districts in this period were anticipated. The results showed an increase in the state component in the 1850s and the 1860s, indicating that the regional difference between the South and the North affected the contestation status of many districts.

The realignment of 1896 led to a further strengthening of the Republican party along a clear regional division: Northeast versus South and West. Figure 1 shows the impact of the realignment as the sharp increase in the state component beginning in the 1890s. A large majority of states ceased to be competitive, with one party or the other establishing predominance around the turn of the century (Burnham 1981b; David 1972).

The New Deal realignment began to undermine the difference in the partisan vote across states from the 1930s. But it was not until the 1950s that the relative importance of state and district components was reversed. After the New Deal, states that had been predominantly Republican gradually became competitive, although predominantly Democratic states did not change much (David 1972). Beginning with the 1950s, Figure 1 indicates a dramatic increase in the district component along with a decay of states as a meaningful level of aggregation. This was caused by the convergence of party strength among states from the 1950s through the 1970s (Sundquist 1983, 344–48). Erosion of the Democratic predominance in the southern states, illustrated by a growing number of successful Republican candidates in statewide and national elections, was especially noteworthy (Ladd and Hadley 1978, 147).

The increase in the district component reflects another important change in the competitive structure of congressional elections. Mayhew (1974) showed that the shape of the distribution of partisan vote in the districts where incumbents were running changed from unimodality centering around 50% Democratic in the 1950s to bimodality centering around 35% and 65% in the 1970s.[5] The bimodal distribution produced greater variance than the unimodal distribution. My analysis shows that the district component was virtually the only factor to shape partisan votes in the 1970s.[6]

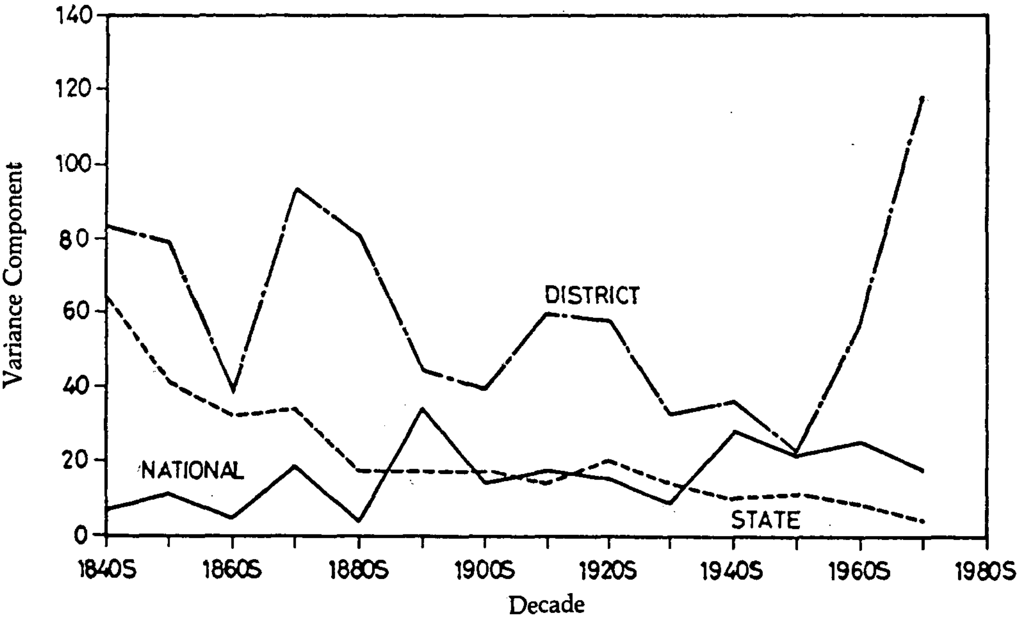
Results—Movement of the Electorate

Figure 2 displays the results of analysis of the movement of the Democratic vote. This is the decadal variance of the swing, partitioned into national, state, and district components. Some proportion of the decadal variance of swing could be attributed to the national component in every decade. But this proportion increased during critical realignments, as shown by the peaks in the 1890s and the 1940s. As mentioned earlier, the decadal variance of swing was smaller in realignment decades. Thus, during realignments there was less variation in swing across the country, and more of that variation was attributable to the national component. These results validate Brady's proposition that in realignment periods "the election results were more national, less local" (1985, 30). Although the configuration of the electorate was never nationalized, the dynamic forces of realignment brought about a degree of uniform, national swing.

Districts responded very sensitively to realignment. In decades of realignment, the district component of swing shrank to almost half what it was at other times. A small district variance suggests that a state moved as a unit toward or away from a major party during realignment. In some states majority parties were overthrown. Although the state component did not change conspicuously over time, states always played a role in a realignment. For example, during the realignment of the 1860s, more than two-fifths of the variance was attributable to the state component.[7]

Thus I find a cyclical pattern to "nationalization as a uniform response," rather than the monotonic nationalization found in previous research. This suggests a reconsideration of the causes of nationalization. The stimulus promoting nationalization cannot be the changing structure of mass communication, as asserted by Stokes (1967). Instead the occasional movement of the vote uniformly across the nation seems to be considered genuinely political. Voters are mobilized nationally when they receive certain cues

Figure 2. Movement of the Democratic Vote



from the national political scene. These affect the subsequent fortunes of the major parties. After the heat of a realignment, each congressman once again tries to secure his or her seat by establishing a personal appeal in his or her district. Consequently the district component grows, reflecting the various degrees of success with which congressmen insulate their districts from the national political scene.

The explosive growth in the district component of swing from the 1950s onward poses an interesting question. My analysis of configuration indicated that districts have become more and more diverse in their partisan outlooks. Now the districts show similar diversity in swing. Burnham (1978) has offered an explanation. He argues that incumbents are given "an increasing, personalized protection" (p. 14). In his "incumbent-insulation model," he predicted how the vote would swing in the three types of districts: in districts where incumbents have served two terms or more, the swing will parallel the national swing but have little effect on the outcome; in districts where freshmen are up for reelection, strong countercyclic swing will favor those of both parties; and in districts where there is no incumbent, strong convergent swing toward 50% will take place (p. 15). The mixture of these swings naturally leads to the maximum heterogeneity of swing across districts. The increased district component my model has found since the 1950s supports Burnham's model.

Conclusion

I have tried to fill the lacuna between electoral analysis of nationalization and realignment. I hope that these results will provoke some further questions about the nature of U.S. party politics. Particularly, the results from the 1950s through the 1970s suggest the need of an in-depth analysis of the electoral dynamics currently at work, which may provide an insight into how they will act in the near future.

I have not undertaken an analysis of turnout. But the criticism I have made of previous conceptualizations clearly suggests the unreliability of the conclusion that turnout is highly nationalized (Claggett, Flanigan, and Zingale 1984; Stokes 1965). Further analysis of the relationship between presidential and congressional elections should also be conducted in light of the finding that critical realignments affect the district, state, and national contributions to voting patterns in elections.

The United States is an extremely diverse nation and this diversity must be captured in an appropriate model of electoral behavior. In a system where the basic voting configuration is determined district by district, the movement of the vote has been affected by both local and national influences. The understanding of voting behavior must reflect this fact.

Appendix: Stokes's Variance-Components Model

Stokes's model is described by the following equation:

$$Y_{ijk} = \alpha + \beta_i + \gamma_{ij} + a_k + b_{ik} + c_{ijk} \qquad \text{(A-1)}$$

where Y_{ijk} is the vote or turnout in the jth district of the ith state in the kth year, α is the fixed national effect, β_i is the fixed effect of the ith state, γ_{ij} is the fixed effect of the jth district of the ith state, a_k is the effect of national forces in the kth year, b_{ik} is the effect of forces in the ith state in the kth year, and c_{ijk} is the effect of forces in the jth district of the ith state in the kth year (Stokes 1965, 66).

Stokes calls this a nested, mixed model. It is nested, because districts are grouped within states and states are grouped within the nation. It is mixed, because some effects within the model are fixed, others random. He stipulates the three random effects as follows:

$$a_k = Y_{..k} - Y_{...} \qquad \text{(A-2)}$$

$$b_{ik} = (Y_{i.k} - Y_{i..}) - (Y_{..k} - Y_{...}) \qquad \text{(A-3)}$$

$$c_{ijk} = (Y_{ijk} - Y_{i.k}) - (Y_{ij.} - Y_{i..}) \qquad \text{(A-4)}$$

Substituting the terms in Equation A-1 by Equations A-2–A-4, we get

$$Y_{ij.} = \alpha + \beta_i + \gamma_{ij}$$

Thus, the model may be described as

$$Y_{ijk} = Y_{ij.} + a_k + b_{ik} + c_{ijk} \qquad \text{(A-5)}$$

I am not interested in following Stokes's precise formulation for the calculation of the national, state, and district components. Instead, I would like to point to some implications of Equation A-5. First, Stokes's model as stated in Equation A-5 is quite close to my model. The difference is that while Stokes's model contains the term $Y_{ij.}$, my model contains M, which is the grand mean of the vote $Y_{...}$. If we put $X_{ijk} = Y_{ijk} - Y_{ij.}$, then Equation A-5 will be $X_{ijk} - a_k + b_{ik} + c_{ijk}$. It follows immediately that $\text{var}(X_{ijk}) = \text{var}(a_k) + \text{var}(b_{ik}) + \text{var}(c_{ijk}) + 2[\text{cov}(a_k, b_{ik}) + \text{cov}(a_k, c_{ijk}) + \text{cov}(b_{ik}, c_{ijk})]$. But because Stokes argues that covariance terms are of negligible importance, $\text{var}(X_{ijk}) = \text{var}(a_k) + \text{var}(b_{ik}) + \text{var}(c_{ijk})$. This is exactly the same as my model except that the left-hand-side expression is the variance of X_{ijk} and not that of Y_{ijk}. Thus, the difference between Stokes's model and my model centers on which variance to partition into national, state, and district components. In my model, the variance of the vote or the swing is to be partitioned. In Stokes's model, the variance of $X_{ijk} = Y_{ijk} - Y_{ij.}$ is to be partitioned. X_{ijk} is the deviation of the vote at a district from the mean over a series of elections. This corresponds to Flanigan and Zingale's (1974) definition of electoral change as movement away from normal voting patterns. Therefore the question is whether to use swing or Flanigan and Zingale's measure to estimate variance components of movement in the electorate. My choice is based on the conviction that swing is the better measure of electoral change, while the vote itself is the best indicator of the configuration of the electorate. Thus, the variance-components model serves to measure the two types of nationalization.

Notes

I would like to thank Michael Les Benedict for always encouraging me and commenting on my work.

1. Clubb, Flanigan, and Zingale (1980) utilized this definition of electoral change extensively. Brady (1985) also depends on a technique based on this definition.

2. The data utilized hereafter is Inter-University Consortium for Political and Social Research (ICPSR) study no. 7757, Candidate and Constituency Statistics of Elections in the United States, 1788–1982 supplied by the ICPSR. The ICPSR bears no responsibility for the analysis or interpretations presented here. A few words about handling procedures are in order. Scholars are divided on whether uncontested districts should be included. Therefore, I did another set of analysis using the data that included uncontested districts. The results showed that there was an overall increase in the level of variance, but over-time trend was not much affected.

3. My model, as well as Stokes's, is sometimes called a *variance-components model,* which is referred to as Model 2 by Kendall and Stuart (1966) and as Model 5 by Graybill (1961).

4. I chose to handle the data in a slightly different way from Stokes. First, I chose the *total vote cast* as the basis for calculating percentages and examined the configuration and the movement of both major parties, because one party is not exactly the mirror image of the other. Second, every contested data available in each decade was used for the calculation of the variance components. This differs from previous approach (Katz 1973; Stokes 1965), which excluded one-district states and included districts only where there were contests throughout the decade being analyzed. Thus, in my analysis the number of cases in the cells relating to year and state is different from one year to another. This is called "complete unbalancedness of the data." Most of analysis of variance programs will handle this kind of data.

5. For arguments related to Mayhew's findings and the behavioral changes of congressmen, see Burnham 1975b; Cain, Ferejohn, and Fiorina 1984; Ferejohn 1977; Fiorina 1977a, 1977b; Hinckley 1981; Jacobson 1983; and Tufte 1973. For arguments from a broader perspective on the transformation of the party system, see Burnham 1975a, Ladd and Hadley 1978; and Sundquist 1983.

6. Configuration of the Republican vote, not shown here, was similar to that of the Democratic vote. However, one point is of interest. There was a relatively large national component in 1912–20. This was probably caused by the Theodore Roosevelt Progressive revolt. In that decade, the national component was 129.41 while the state and district components were 101.81 and 196.20.

7. The results of analysis on the movement of the Republican vote were only slightly different. Thus the movement of the Democratic vote tells most of the story. But there was one major difference. In 1912–20, the state and district components jumped in the Republican analysis, while the district component did and the state component did not increase in the Democratic analysis. Burnham, Clubb, and Flanigan (1978) found a similar increase in an analysis of state averages of absolute values of first differences of the Republican proportion of the vote for congressmen. Their explanation seems pertinent to my result. That is, high scores in the 1912–20 reflect the Bull Moose schism in the party, and they will become smaller by combining vote returns of the old guards with those of the Progressives.

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DEMOCRATIC REPRESENTATION AND PARTISAN BIAS IN CONGRESSIONAL ELECTIONS

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The translation of citizen votes into legislative seats is of central importance in democratic electoral systems. It has been a longstanding concern among scholars in political science and in numerous other disciplines. Throughout this literature, two fundamental tenets of democratic theory, partisan bias and democratic representation, have often been confused. We develop a general statistical model of the relationship between votes and seats and separate these two important concepts theoretically and empirically. In so doing, we also solve several methodological problems with the study of seats, votes, and the cube law. An application to U.S. congressional districts provides estimates of bias and representation for each state and demonstrates the model's utility. Results of this application show distinct types of representation coexisting in U.S. states. Although most states have small partisan biases, there are some with a substantial degree of bias.

The relationship between legislative seats and citizen votes is a longstanding concern in democratic theory (e.g., Balinski and Young 1982; Dahl 1956, 147–49; Farrand 1911; Locke 1965, 419–20; Rae 1967; Schattschneider 1942). Through this relationship, legislative majorities are formed and minorities protected. Constitutionally mandated reapportionment and shifting patterns of partisanship have created opportunities for state legislatures and partisan gerrymanders to alter the congressional seats-votes relationship (Cain 1984; Grofman et al. 1982; Polsby 1971). Over the last century, scholars in political science, sociology, economics, mathematics, statistics, and political geography have studied these normative theoretical questions and sought empirical estimates of bias and unfairness (Hay and Rumley 1984; Kendall and Stuart 1950; March 1957–58; Theil 1970; Tufte 1973). Furthermore, the recent spate of court challenges, the courts' willingness to hear political gerrymandering cases, and the Supreme Court's interest in a threshold of political discrimination, have rekindled the seats-votes controversy (Karcher v. Daggett, 462 U.S. 725 [1983]; Davis v. Bandemer, 478 U.S. [1986]; Brown v. Thomson, 462 U.S. 835 [1983]).

Concern with and, we believe, confusion over two fundamental tenets of democratic theory, partisan *bias* and democratic *representation,* dominate this literature. Partisan bias introduces asymmetry into the seats-votes relationship, resulting in an unfair partisan differential in the ability to win legislative seats: the advantaged party will be able to receive a larger number of seats for a fixed number

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of votes than will the disadvantaged party. Although *bias* is easily defined, it is not always as apparent or as easily measured (Grofman 1983). Even in the absence of partisan bias, several forms of democratic representation are possible: strict proportional representation—in which the percentage of seats equals the percentage of votes—and winner-take-all elections are the pure forms, with many other possibilities in between.[1] Whereas the extent of bias is a separate problem, the precise effect of partisan bias depends on the specific form of democratic representation.

Much of the literature either treats partisan bias and democratic representation as one concept or mistakenly confuses different democratically legitimate forms of representation with clearly invidious partisan bias. Because bias and representation are related, the separate estimations in most previous research can be shown to be statistically inconsistent. By expressing each of these concepts as a separate parameter in a unified model, we show that it is possible and useful to emphasize the analytical and empirical distinctions by developing a model that jointly estimates both of these parameters. In a single and conceptually simple equation, we incorporate the full range of possible values for bias and representation. The result is a general form that is useful in understanding both the fairness of legislative reapportionment and the democratic character of legislative representation. The validity and utility of this model is then demonstrated with an application to state level congressional seats and votes for the period 1950–84.

The essence of bias and representation in democratic regimes is realized in the translation of votes into seats.[2] Assume initially that there are only two parties, Democratic and Republican, and that the legislature is composed of a set of single-member, winner-take-all districts. We begin with a few standard definitions. Let

v = the number of votes cast for Democratic party candidates

T = the total number of votes cast for candidates of both parties

v_R = $T - v$ = the number of votes cast for Republican party candidates

V = v/T = the proportion of votes cast for Democratic candidates

V_R = $1 - V$ = the proportion of votes cast for Republican candidates

s = the number of seats allocated to the Democratic party candidates

s_R = the number of seats allocated to the Republican party candidates

D = the total number of single-member legislative districts

S = s/D = the proportion of seats allocated to the Democratic candidates

S_R = $1 - S$ = the proportion of seats allocated to the Republican candidates

We express the absence of partisan bias as partisan symmetry. In general, this means that in an election system where $x\%$ of the Democratic votes produces an allocation of $y\%$ of the seats to the Democrats, then in another election under the same system $x\%$ of the Republican votes would yield the same $y\%$ Republican allocation of seats. This is the situation that Grofman (1983) calls "completely unbiased."

Stated more formally, if $V = x \Rightarrow S = y$, then $V_R = x \Rightarrow S_R = y$, for all x and y. This completely unbiased system requires only one point at which the percentage of votes equals the percentage of seats: when each party receives 50% of the votes, the seats must be divided equally between them. The partisan fairness expressed by this symmetry does not restrict x to equal y at any but this point. When it is true that $x = y$, for all x and y, we have the situation of unbiased proportional representation. However, there are many other interesting types of unbiased representation systems, and our model explicitly incorporates the full range of these.[3]

Bias, formalized as partisan asymmetry, makes it possible for one party to receive 50% of the votes but not necessarily 50% of the seats. This situation is modeled similarly: If $V = x \Rightarrow S = y$, then $V_R = x \Rightarrow S_R = z$, where y is not necessarily equal to z. However, in the United States, even a biased system would not allocate any seats to a party without any votes. This means that both biased and unbiased systems are restricted to pass through the (0%, 0%) and (100%, 100%) points on the votes-to-seats curve; it is near the middle range of votes and seats that the potential for bias is greatest.

Our model of representation and bias will now be developed and explained in more detail. We introduce the mathematical form and explain the substantive significance of the bias and representation parameters of each. Those preferring a nonmathematical exposition are referred to Figures 1–3.

Modeling Representation

There are a number of plausible functional forms that could be used to model the full range of representation while still restricting the system to be unbiased. Most of these forms lead to nearly identical conclusions, if not to the same models. We believe our model best matches the definitions above and has the additional advantage of being a generalized form of the best-known model of the votes-seats relationship, the "cube law" of electoral politics. Our generalization also extracts the hidden features of this formal "law," known at least since 1909 (see Kendall and Stuart 1950), and expresses them in a more interpretable form. We show how this form can realistically and flexibly model concepts and relationships of fundamental importance to democratic theorists, political scientists, and the courts.

Equation 1, with ρ set equal to 3, is the classic cube law:

$$\left(\frac{S}{1-S}\right)=\left(\frac{V}{1-V}\right)^{\rho} \qquad (1)$$

Some time after its inception, investigation with actual election results indicated that values for ρ other than 3 were better descriptions of many electoral systems (see Taagepera 1973; Tufte 1973; and the citations in Grofman 1983, 317). While this mathematical relationship is straightforward, it is difficult to interpret in a theoretically meaningful way without either knowledge of analytical geometry or specific applications.

As an alternative, consider the algebraic characterization that follows. First, by taking natural logs, we rewrite Equation 1 as

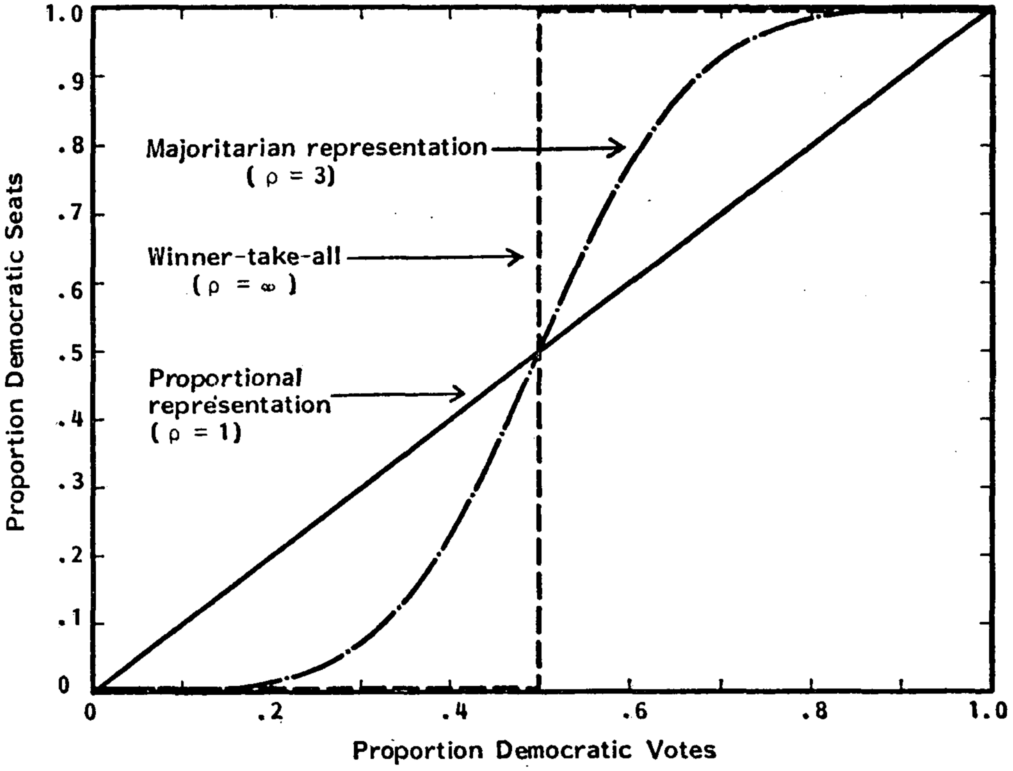
$$\ln\left(\frac{S}{1-S}\right)=\rho\ln\left(\frac{V}{1-V}\right) \qquad (2)$$

Tufte (1973, 545), making unrealistic assumptions about the disturbance term (see Linehan and Schrodt 1978), estimated ρ by running a linear regression of $\ln[S/(1-S)]$ on $\ln[V/(1-V)]$ and including a constant term in the equation.[4] With some additional algebraic manipulation, Equation 2 can be expressed as a modification of the dichotomous logit model (see King 1986a). Thus, from the perspective of models common in political science, Equation 3 should be more directly interpretable:

$$s = D\left\{1+\exp\left[-\rho\ln\left(\frac{V}{1-V}\right)\right]\right\}^{-1} \qquad (3)$$

There are two differences between Equation 3 and the logit model commonly used to analyze dichotomous dependent variables: $\ln(V/1-V)$ is a log-odds function of V; and there is no constant term.

Figure 1. Forms of Unbiased Representation (Based on Equation 3)



However, the inverse of the term in braces still ranges between 0 and 1, and, when multiplied by D, the entire right-hand side is restricted to vary between 0 and D, the number of districts. Since a log-odds transformation is the inverse of a logit, we call Equation 3 the *bilogit* functional form. Thus, we have in Equation 3 a model that generates the forms of representation depicted in Figure 1.[5]

Figure 1 demonstrates the range of functional forms that can emerge from Equation 3 and depend on the value of ρ (rho), the representation parameter. We discuss winner-take-all representation ($\rho = \infty$), majoritarian representation ($1 < \rho < \infty$), and proportional representation ($\rho = 1$). Equation 3 can also be used to model antimajoritarian or unresponsive representation ($0 < \rho < 1$), not discussed here.

When $\rho = 1$, the translation of votes into seats is by *proportional representation*. As the figure indicates, some proportion of votes will yield exactly that proportion of seats for the Democratic party. Exact proportional representation is unlikely in actual U.S. district-based elections, if only because there are fewer seats than votes: it would require a proportional increase in seats ($1/T$, to be precise) for even one additional vote; this is impossible, unless there were one seat for each voter. Other more realistic conditions, such as incumbency and party competition, can also lead to results that are not proportional.

Majoritarian representation is the situation where $1 < \rho < \infty$. The further ρ is from 1, the further the electoral system is from proportional representation. A common example of this is the cube law ($\rho = 3$), portrayed as the curve in Figure 1 in the shape of an escalator. This relationship between seats and votes helps majorities to form. When a party approaches 50% of the votes, each additional increment of voters increases the proportion of seats by a larger amount, as evidenced by the steep slope at $V = .5$); thus, parties are encouraged in their search for majorities. There is also an incentive for more partisan competition, since the marginal benefit, in terms of seats, of an additional increment of votes is greatest as both parties approach 50% of the votes. But one aspect of this relationship has been overlooked: although this form of representation favors majority and near-majority parties, there is a sense in which it protects minorities. This can be seen by looking near the top and bottom of the graph, where the slope of the line becomes progressively flatter. After a party gets 50% of the vote (and a majority of seats), each additional increment of votes yields a smaller incremental proportion of seats. The increase in the proportion of majority-party voters it would take to eliminate the last percentage point of minority seats (i.e., from 99% to 100% for the majority party) is far greater than the increase in voters it would take to reduce minority representation by one percentage point near the middle of the curve (say, from 55% to 56% for the majority party). This majoritarian electoral system thus encourages majorities to form but simultaneously makes it more difficult for minority representation to be eliminated. Although only one type of majoritarian representation is pictured in Figure 1, there are an infinite number of possibilities—from just beyond proportional representation ($\rho > 1$) to just before winner-take-all ($r < \infty$).

A third situation is *winner-take-all*, which occurs when $\rho = \infty$. This is also portrayed in Figure 1. In this case, 50% plus one vote translates into 100% of the seats. Although this situation exists for each congressional district, for example, it does not usually apply to aggregates of them.[6]

Although proportional representation is most often proffered as the standard of fairness, we see no a priori reason to believe that one form of representation is inherently more fair than the others, provided that there is partisan symmetry. Convincing arguments can be made in favor of each of these types of democratic representation. At first glance proportional representation seems fair, since the translation process *reflects* underlying voter preferences most directly. But representation systems need not only reflect to be fair and meaningful (King and Ragsdale 1987; Pitkin 1967). Winner-take-all systems, for example, have some elements of reflection but also recognize that only one party can, and assume that only one party should, govern. These electoral systems thus emphasize ability to govern *and* reflection in the method of translating citizen votes into legislative seats. In general, there is a trade-off between these two criteria, but since winner-take-all systems do not favor one political party over the other, there is no real reason to consider it unfair. In fact, one can argue that majoritarian representation, falling between proportional and winner-take-all representation, best describes many popular notions of U.S. democracy: majorities are encouraged, but small minorities are protected and thus represented. An "optimal" value of ρ is therefore a matter for political or judicial decision. Thus, there appears to be no a priori or axiomatic basis on which to choose one system over another.[7]

Modeling Bias

Although there are many types of "fair" democratic systems of representation, partisan bias is usually condemned. Unfortunately, although partisan bias is often discussed, it is seldom estimated. Indeed, it is not even included as part of the cube law. In order to incorporate the possibility of bias into this model, we must choose a form that still restricts the votes-seats curve to pass through the (0,0) and (1,1) points; this means that a party with no votes will receive no seats. At the same time, this model must still allow for the full range of forms of representation already explicated.

Our solution is to augment Equation 1 (reinterpreted as Equation 3) with a bias parameter; this allows the joint estimation of both partisan bias and democratic representation. Letting β (*b*eta) be the *b*ias parameter, the new generalized cube law can be written as[8]

$$\left(\frac{S}{1-S}\right) = \beta\left(\frac{V}{1-V}\right)^{\rho} \tag{4}$$

This form allows the curves drawn in Figure 1, from Equation 3, to be asymmetric, our definition of partisan bias.[9]

Almost every empirical study of the cube law has implicitly assumed that $\beta = 1$, the situation of no bias. When there is in fact no bias, the $\beta = 1$ constraint causes no problem. However, when there is bias toward one of the political parties, the constraint will result in statistically inconsistent estimates. The model in Equation 4 allows for bias without the possibility of statistical inconsistency. Tufte (1973) was probably the first to recognize that bias and representation could be modeled in one equation, and his was a linear approximation to Equation 4. We believe our nonlinear model is a more realistic version than Tufte's in that we allow for every possible degree of partisan bias and every possible form of democratic representation. Unlike the linear model, even systems with widely varying and quite extreme values of S and V can be incorporated in this model.

Equation 4 can also be written in an algebraically equivalent, but more substantively interpretable, bilogit form:

$$s = D\left\{1 + \exp\left[-\ln(\beta) - \rho\ln\left(\frac{V}{1-V}\right)\right]\right\}^{-1} \tag{5}$$

We incorporate the bias parameter in this model because of widespread concern about the fairness of the congressional reapportionment process. We therefore provide a more realistic model of both bias and representation, allowing for the exact form of the bias to depend upon the specific type of electoral representation. Consider now what happens when bias is added to the unbiased forms of representation pictured in Figure 1.

Generically, bias refers to asymmetry in the seats-votes relationship for the two parties. The easiest type of bias to understand is for winner-take-all systems ($\rho = \infty$). A bias in favor of the Republicans is the case where the discontinuity in the curve (the vertical part of the line in Fig. 1, $\rho = \infty$) is moved to the right: the proportion of the vote that the Democrats would have to win in order to take all the seats would be greater than half. Similarly, if the discontinuity moved to the left, the bias would help the Democrats. Bias in winner-take-all systems is so apparent that it rarely occurs in U.S. elections, except in the presence of fraud or other abuses.[10] The particular forms of bias associated with the other two types of representation are quite distinct and more complex. The empirical results presented below indicate that bias coexists with many forms of representation in a number of U.S. states.

Table 1 summarizes the possible ranges

of the bias parameter and their respective interpretations. Since β is log-symmetric (ranging from 0 to ∞ with 1 at the center), it will be convenient to express the coefficient in terms of its natural log, that is $\ln\beta$ (ranging from $-\infty$ to ∞ with 0 at the center).

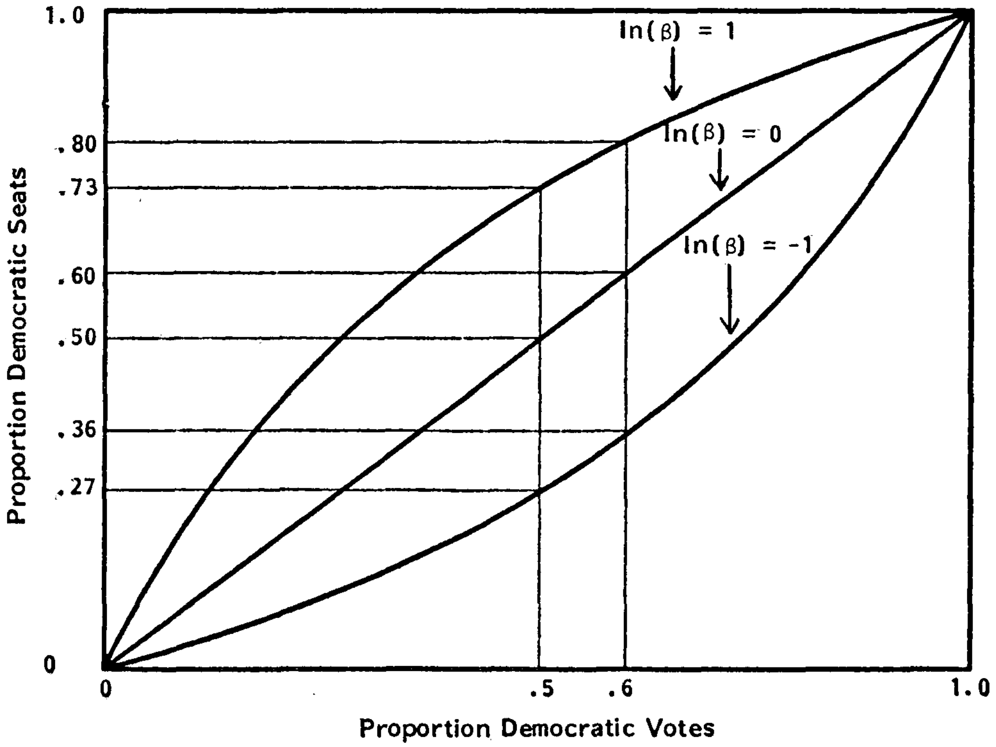
Figure 2 plots three types of bias under a proportional representation system.[11] $\ln\beta = 0$ is obviously the case of no bias: .5 Democratic votes yields .5 Democratic seats; .6 Democratic votes yields .6 Democratic seats. This is proportional representation because the proportion of seats equals the proportion of votes; it is unbiased because these same figures also hold for the Republican party. The 45° line embodies this relationship; a given percentage change in seats yields an equal percentage change in votes throughout. In evaluating the other two biased lines, it is useful to consider this unbiased plot as the baseline.

Table 1. Bias Coefficient Values

Coefficient Value	Direction of Bias
$\ln\beta > 0 \Leftrightarrow \beta > 1$	bias towards Democrats
$\ln\beta = 0 \Leftrightarrow \beta = 1$	unbiased
$\ln\beta < 0 \Leftrightarrow \beta < 1$	bias toward Republicans

For example, under proportional representation, .5 Democratic votes should

Figure 2. Bias and Proportional Representation (Based on Equation 5)



yield .5 Democratic seats (because this is where the vertical line meets the unbiased line), but the line marked $\ln\beta = -1$ only allocates .27 Democratic seats and the line marked $\ln\beta = 1$ allocates .73. A similar situation exists at all other points on this graph. At .6 Democratic votes, the fair outcome is .6 Democratic seats, but the upper line, $\ln\beta = 1$, is biased toward the Democrats, yielding .8 of the Democratic seats. The lower line, $\ln\beta = -1$, is biased toward the Republicans, yielding only .36 Democratic seats. The asymmetry defines our theoretical notion of bias.

Note that for each point on the horizontal axis, there is a different absolute bias for different parts of any biased line. For $\ln\beta = 1$, the absolute bias is $|.5 - .73| = .23$ at .5 of the Democratic votes, but it is only $|.6 - .8| = .20$ at .6 votes Democratic. In our model, the maximum absolute bias occurs at or near the .5 mark because the curves converge as they approach 0 or 1. This provides some justification for formal statistical models (Quandt 1974) and more intuitive analyses (Tufte 1973) based on bias measured only at .5. However, since there are many electoral systems where the percentage Democratic is rarely near 50%, it also suggests that we should look past this point to incorporate the full range of bias. For proportional representation systems, a measure based on the Gini index of the area between the biased and unbiased curves is possible, but this does not generalize as easily to the majoritarian representation or winner-take-all cases. Grofman's (1975, 1983) "normalized measure" of bias could be utilized here, but for present purposes the most natural way to measure the range of bias existing in the system is to use β or, equivalently, $\ln\beta$.

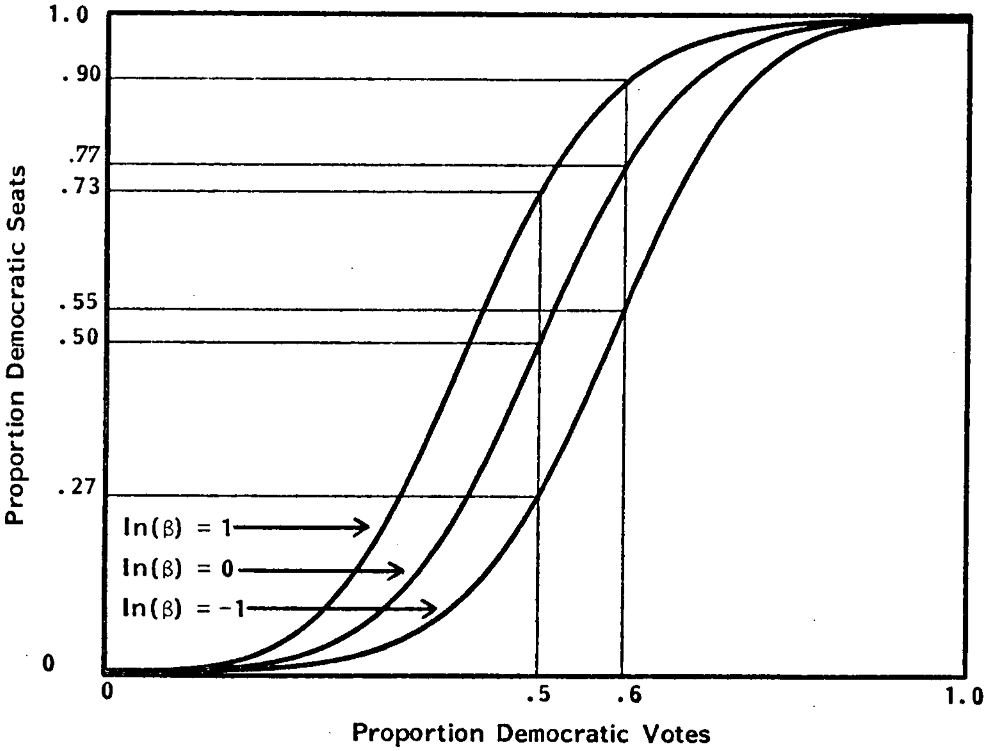
Figure 3 expresses bias for majoritarian representation systems of the specific type $\rho = 3$. The line in the middle, marked $\ln\beta = 0$, is the unbiased line included for reference. At .6 votes Democratic, the fair proportion of Democratic seats is .77. This is fair under the $\rho = 3$ majoritarian system, but not necessarily under other representational schemes. For the $\ln\beta = -1$ line, there is a bias favoring the Republicans, so that the same .6 votes Democratic yields only a .55 proportion of Democratic seats. The $\ln\beta = 1$ line, biased toward the Democrats, yields .9 of the Democratic seats. For this figure, the absolute bias is also different for each point on the horizontal axis. For the line marked $\ln\beta = 1$, the absolute bias at .5 votes Democratic is $|.5 - .73| = .23$, but the absolute bias at .6 is only $|.77 - .90| = .13$.[12]

Deterministic Laws and Probabilistic Realities

Some of the most significant contributions to the literature on seats-votes relationships imply that the cube law, or some relevant variant, is deterministic. Whether this relationship is deterministic or probabilistic is an empirical issue, and it has important consequences for theoretical understanding and data analyses. We believe it is difficult to find even one meaningful example of a deterministic law anywhere in the social sciences.[13] There is also strong evidence that a deterministic relationship between seats and votes does not exist: "While the cube law is stated deterministically, even a cursory examination of election statistics shows that it does not hold perfectly" (Schrodt 1981, 33). Michels's (1911) "Iron Law of Oligarchy" and many others have also failed the test. In making a general point (coincidentally using the cube law as an example), Achen (1982, 15) writes, "Any attempt at specifying exact causal functions must necessarily result in oversimplified explanations."

The Appendix develops a number of original, but somewhat technical, points: we justify a binomial disturbance term and add it to Equation 5, propose a

Figure 3. Bias and Majoritarian Representation (Based on Equation 5; $\rho = 3$)



method of estimation, and provide some empirical estimates. The sections that follow use the results from the Appendix.

Data and Measures

To demonstrate the empirical utility of this model, it is applied to data on U.S. congressional elections, 1950–84.[14] For most analyses of the U.S. Congress, the entire nation has been used. But there are significant findings that demonstrate substantial variation across states (e.g., Scarrow 1983; Tufte 1973). We will therefore conduct separate analyses for each state, using the 18 elections between 1950 and 1984. Alaska and Hawaii were excluded because they were not states during the entire period. Small changes in the elections included in the analysis did not materially alter the results because of the generally slow changes in state electoral systems.

The implied assumption in this approach is that β and ρ vary more across states than over time within any one state. We believe that this is justifiable on two grounds. First, the whole process of reapportionment is conducted separately by each state legislature. Aside from significant variation in the decisions by state legislatures, there are also large discrepancies in political geography and political culture among the states. Surely, the variation over time would be less than these cross-state differences. Second, we report large variations in both the degree of bias and the type of representation across states (see Figs. 4 and 5). At the same time, empirical tests indicated substantial stability within states over time.[15] If β and ρ do vary more than expected over time for a particular state, then our estimates are average values for that state.

These data were coded from, and cross-checked against, the *Statistical Abstract of the U.S.* (annual volumes), Congressional Quarterly's (1975) *Guide to U.S. Elections*, Cox's (1972) *State and National Voting, 1910–1970*, and Scammon and McGillivray's *America Votes* (annual volumes). As Niemi and Fett (1986) point out, the data collection is not as straightforward as it might seem. For example, we delete the very few representatives who had won seats under the independent-party label and subtracted the votes received by their Democratic and Republican opponents from the statewide total. We ignore resignations,deaths, and special elections. The member receiving the most votes in the general election was presumed to be elected. At-large districts, used by a few states immediately following reapportionment, are excluded unless there is only one district in the state. Several states have laws that do not require votes to be tabulated in uncontested races. For these states, we included only those districts and votes that were contested and reported. All other districts and votes are included.

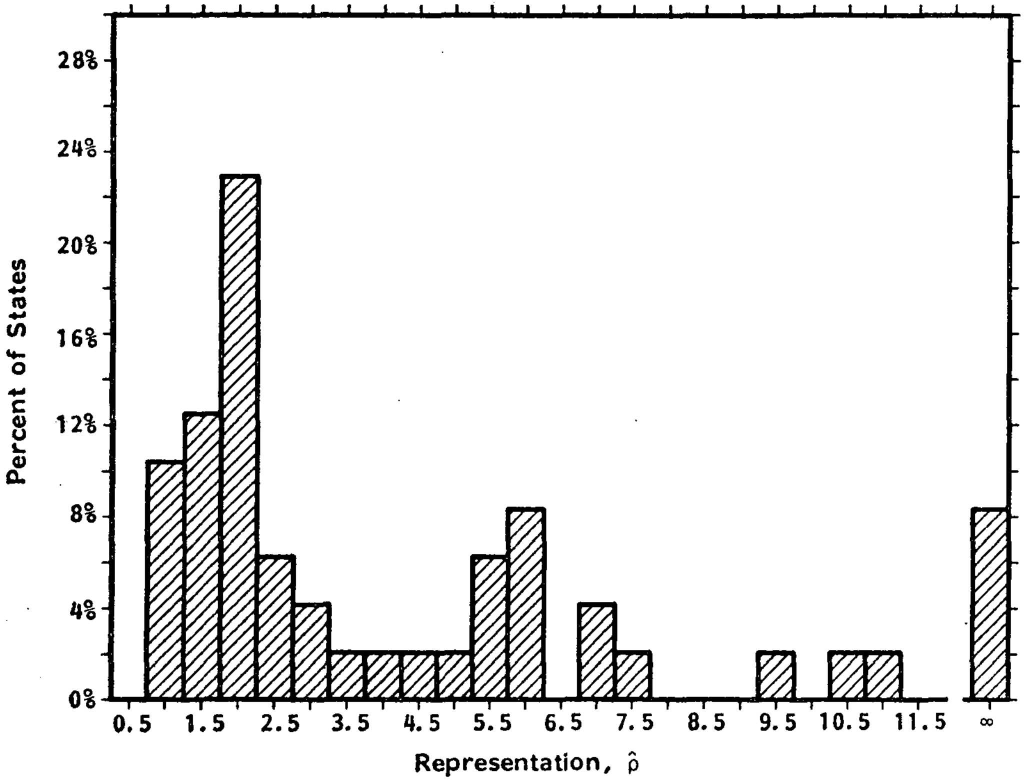
Some have argued that the form of democratic representation is a function of the number of districts and voters, the geographical distribution, party competition, and the number of incumbents. Our approach follows two steps. We first estimate the bias and representation parameters for each state using the number of votes and seats for each party and election from 1950 to 1984. Once these parameters have been estimated, we show how differences in these estimates across states can be explained by measures of state political characteristics. This two-stage approach is as good as a simultaneous estimation because β, ρ, and many of these exogenous state characteristics change considerably more across states than over time.[16]

Estimating Representation and Bias in U.S. House Elections

Using Equation 5 as the general model, the estimation procedure described in the Appendix, and the data introduced below, bias and representation coefficients were estimated for each state. We present the results in Table A-1. For easier interpretation, Figure 4 presents a histogram of the representation coefficients.[17] Note that the distribution of these coefficients are trimodal, with modes at or near 1, 6, and 10. Most of the states are quite near to proportional representation, but significant numbers are strongly majoritarian, and several are approximately winner-take-all. Note that although ρ ranges up to infinity, ρ coefficients greater than 8 or 9 are essentially winner-take-all.

This finding demonstrates the utility of the model in three ways. First, it shows the large variance in the type of representation across states. Second, it demonstrates that the plurality of states are just above proportional representation ($\rho = 1$). Although a number of studies have shown that ρ is not equal to 3, the cube-law value, most find that ρ falls between 2 and 4, with an average of about 3. Finally, the results indicate that previous estimation procedures may have been statistically inconsistent due to the exclusion of a bias parameter. The extent of inconsistency in previous research is also quite substantial: although deriving an analytical expression for the inconsistency appears intractable, it is possible to get a feel for the extent of the problem. To

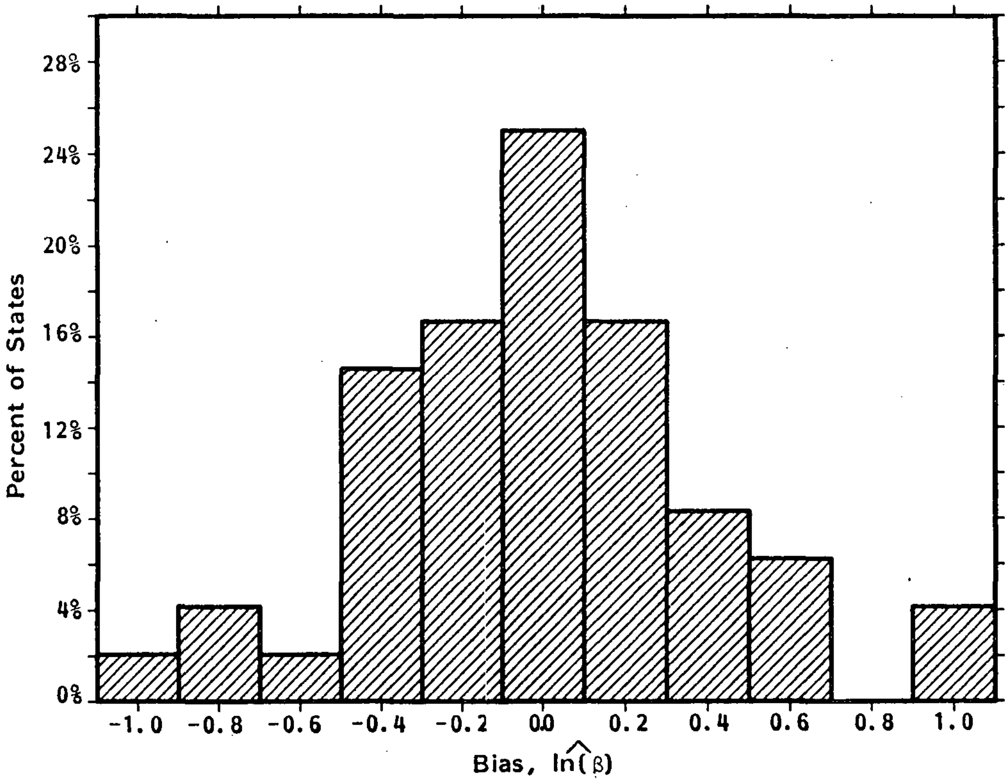
Figure 4. Representation in U.S. States



accomplish this, the state bilogit estimations were also run while constraining $\beta = 1$ [i.e., $\ln\beta = 0$]—exactly as if the bias parameter had not been included. In these constrained estimations, ρ was too large about 1.5 times more often than it was too small. It thus appears that previous research has overestimated the degree to which U.S. democracy tended away from proportional representation and toward majoritarian representation. Although these are the tendencies, in any particular example it is unclear whether the inconsistency will cause the estimate of ρ to be too large or too small; the proper way to provide a consistent estimate of ρ is to use the joint model and estimation method proposed here.[18]

There is also substantial variation in the degree and direction of bias in U.S. congressional elections across states. Figure 5 presents a histogram of the estimated β coefficients. Note that the mean is almost exactly 0, and there is an approximately symmetric normal distribution around this point. This implies that the average of the states is not too biased toward one party more than the other. However, not all points fall on or about $\ln\beta = 0$, suggesting that at least some bias exists in individual states. In fact, even allowing for deviations from unbiasedness due to sampling variation and measurement error, the point estimates indicate that some states have quite large biases (see Fig. 5). There are two well-defined groups

Figure 5. Bias in U.S. States

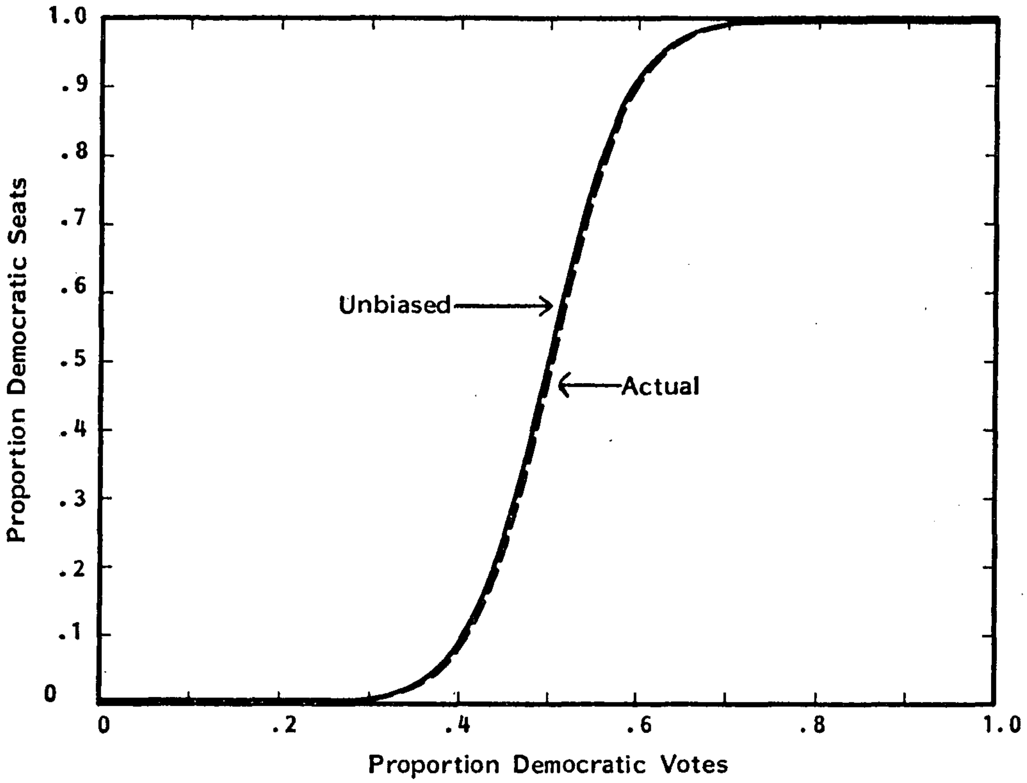


of outliers at the ends of Figure 5. Kansas, Michigan, and Ohio have a substantial Republican bias, whereas Texas, California, and Florida have a substantial Democratic bias.[19]

The effect of bias on proportional and majoritarian representation systems is seen in the examples of Indiana and Texas, graphed in Figures 6 and 7, respectively. Indiana is a generally Republican, but still competitive, two-party state. Our results indicate a very slight Republican bias (the unbiased baseline is almost indistinguishable from our empirical findings) and a steep, majoritarian, seats-votes relationship (the unbiased baseline is a steep curve with two sharp bends). Indiana and other strong party states tend to demonstrate the slight bias and steeper slope portrayed in this figure.

Representation in Texas is quite near to proportional. The no-bias baseline in Texas is the nearly straight-line proportional relationship (see Fig. 7). However, this baseline deviates substantially from Texas electoral politics; the dominant Democratic tradition has created quite severe biases toward the Democrats, permitting them to win a majority of the seats with less than 30% of the votes. This is an extreme example of the biases that do exist in U.S. politics.

Figure 6. Bias and Representation in Indiana ($\hat{\rho}$ = 5.70, ln($\hat{\beta}$) = −.08)

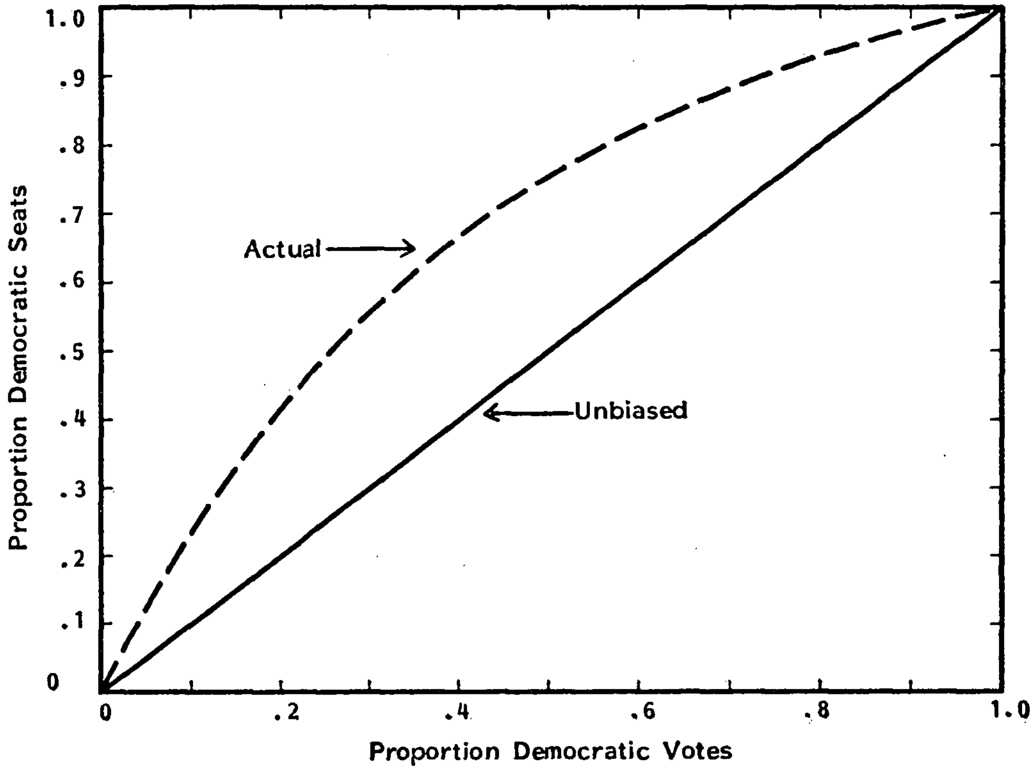


Explaining Representation and Bias in the U.S. House

In this section, we explore the differences among states in the bias and representation coefficients generated previously. This analysis helps to validate the estimates produced there. Leaving out the states with too little data for estimation or with coefficients equal to infinity (indicating winner-take-all representation), two series (bias and representation) of 44 observations each remain. The parameter estimates have different expected values (asymptotically equal to the population parameters) across states. The variation around these expected values is likely to be different for each coefficient, resulting in heteroscedastic disturbances. Fortunately, we can estimate these variances by the approximate squared standard error resulting from the first-stage analysis. Furthermore, since the first-stage analysis was estimated with maximum likelihood, these coefficients will be normally distributed.

Taking all these factors into account, we use a weighted least-squares analysis, regressing the estimates of representation and bias on separate sets of explanatory variables.[20] Consider representation first. There are two main explanations for variation in the representation parameter. The best justified is Taagepera's (1973) index, ln(T)/ln(D), where T is the total number of voters and D is the number of districts. To understand the logic behind this index, it is useful to focus on the

Figure 7. Bias and Representation in Texas ($\hat{\rho} = 1.05$, $\ln(\hat{\beta}) = 1.12$)



extreme cases. When there is no bias and one district for each voter (i.e., $T = D$), $x\%$ of the votes for one party will automatically yield $x\%$ of the seats for that party. This proportional representation result is captured by Taagepera's index, since $\ln(T)/\ln(D)$, where $T = D$, is equal to 1. The winner-take-all extreme is also captured by this index: When there is only one district for all the voters, as in a presidential election, we have $\ln(T)/\ln(1) = \ln(T)/0 \approx \infty$. Thus, the extreme values of this index are theoretically appropriate. Between these two extremes, the index predicts a gradual and continuous increase in the representation parameter—from proportional ($\rho = 1$) to majoritarian ($1 < \rho < \infty$) to winner-take-all ($\rho = \infty$) forms of representation—as the number of districts increases relative to the number of voters. The precise rate at which ρ increases is defined by the form of his index, $\ln(T)/\ln(D)$. Other forms that also meet the boundary conditions are possible, but Taagepera's (1973) seems plausible. In the weighted-least-squares regression analysis performed here, this hypothesis would be confirmed if the regression coefficient on this index were 1.0 and the coefficients on the other variables in the equation were 0.

Party competition has been suggested as an examination for the form of representation. In strongly competitive party systems, where the percentage voting for each party in each election district is near 50%, a small increase in votes for one party across districts will likely result in a

Table 2. Explaining Representation

Independent Variable	Estimated Coefficient	Standard Error	t-Statistic
Constant	-1.17	.67	—
Taagepera's index	.32	.07	4.62
Party competition	2.17	.70	3.09

Note: Number of observations (states) = 44; standard error of the weighted-least-squares regression = .82; mean of the dependent variable ($\hat{\rho}$) = 1.54.

large increase in seats for that party. Party competition also encompasses the effect of the power of incumbency and uncontested seats. From the widely used Ranney index of state party strength (Ranney 1976), we construct a measure of party competition that ranges from 0 (no competition) to 1 (pure competition).[21]

Table 2 presents the weighted least-squares regression of our estimated representation parameters on Taagepera's index and party competition. The estimates for the effect of both variables are relatively precise and, at conventional levels, significantly different from 0. Taagepera's index has an effect about one-third of what his theory would predict, but it has the correct sign, and helps to explain interstate variation in the form of representation. This finding has several interpretations: a different form of the index might account somewhat better for the range of representation types between proportional and winner-take-all, or there may be a tendency for U.S. states to be more proportional than would be expected solely on the basis of the relative sizes of their voter populations and numbers of legislative districts. Both the value of, and the need for further research on, Taagepera's index are emphasized by this result.

The effect of party competition is shown to be relatively strong: if a state were to move from the lowest to the highest level of partisan competition, the representation parameter would move more than two points toward the winner-take-all extreme (see Table 2). This is well within the bounds of the original hypothesis.

The propensity of a state to be biased toward the Republicans or Democrats has generally been explained by relative party strength. Parties may be in decline in some ways, but the desire to gerrymander remains as strong as ever.[22] We would therefore expect all parties in all states to attempt to gerrymander, but only the states with dominant party systems would be successful.

As a measure of party strength, we use the Ranney index, ranging, as usual, from 0 (Republican) to 1 (Democratic). We also use a measure of state ideological orientation ranging from −1 (liberal) to 1 (conservative), estimated from state level CBS News–*New York Times* polls by Wright, Erikson, and McIver (1985).

The weighted-least-squares-regression results of estimated bias on state party strength and state ideological orientation are reported in Table 3. Both coefficients are in the hypothesized directions, although the ideology coefficient does not meet conventional significance levels. Our results indicate that the strongest Democratic state has a bias coefficient about .9 points higher (in the direction of Democratic bias) than the strongest Republican state, and that ideologically liberal states tend to be somewhat more biased toward the Democrats.

The plausibility of these second-stage results lends criterion validity to our original model. Variations in our estimates of bias and representation in the U.S. states vary relatively closely with

Table 3. Explaining Partisan Bias

Independent Variable	Estimated Coefficient	Standard Error	t-Statistic
Constant	−.44	.20	—
Party strength	.90	.32	2.82
Ideology	−.67	.59	−1.15

Note: Number of observations (states) = 44; standard error of the weighted-least-squares regression = .64; mean of the dependent variable ($\hat{\rho}$) = −.05.

these plausible criterion variables. Sensitivity analyses indicate that these findings are quire robust to marginally different specifications.[23]

Conclusion

In this paper, the venerable cube law of electoral policies was reformulated, generalized, and reinterpreted to provide a model for the relationship between votes and seats in representative democracies. Whereas Taagepera (1986) generalized this law to proportional-representation elections, we generalize it to include both the form of democratic representation and the extent of partisan bias. Previous analyses tended to confuse the form of representation with the degree of partisan bias and have unintentionally constrained the partisan-bias parameter, in effect assuming no bias.

One consequence of this is that unbiasedness has mistakenly been associated with proportional representation. We find that all representation types captured by a single parameter in our model can treat the parties symmetrically using our definition of unbiasedness. Recent court rulings have upheld this view.[24]

Additionally, by constraining the β parameter, past research has yielded statistically inconsistent estimates of the representation parameter, ρ, ranging between 2 and 4 for most electoral systems. In contrast, we find that the plurality of states were much closer to proportional representation than had been previously believed. Nevertheless, it is not true that all previous estimations of representation can simply be slightly adjusted toward 1.0. Although the empirical tendency is in this direction, the direction and size of the inconsistency is unknown for any particular example. Thus, some previous analyses may be overestimates and some may be underestimates. Statistical consistency requires that partisan bias be incorporated as an additional parameter. After all, if a party receives, for example, 55% of the votes and 75% of the seats, this may represent a severe partisan bias or a fair system with majoritarian representation. The only way to distinguish between the two situations is to have joint estimates of both β and ρ.

The remarkably flexible bilogit functional form helped to highlight these problems with past research in the context of a theoretically meaningful solution. The model developed herein also demonstrates that different forms of representation are associated with different forms of bias, and that partisan bias can occur in widely varying degrees. This paper has also contributed to the statistical estimation of this inherently nondeterministic relationship between seats and votes. Applying a discrete probability distribution to the discrete variable, s, the number of Democratic seats, is a considerably more statistically efficient procedure than had previously been used. In an application to congressional elections, we demonstrate that there exists a wide variety of representation systems and degrees of partisan bias. These variations have important implications for national politics and policy (Browning 1986).

Finally, a second stage of the analysis provided some validity to the estimates from the first stage and some evidence that there are systematic explanations for the direction and size of the bias and representation parameters. The level of state party competition and Taagepera's index of state and electorate size helped to explain differences in the representation parameter estimates across states. The Ranney index of party strength and a measure of the state electorate's ideological orientations indicated when and where bias was more likely to occur.

Beyond these contributions, these results also have important implications for the recent court cases on reapportionment. First, the courts must explicitly distinguish between bias and representation type. The discussion of using seats and votes to create indicators of political discrimination in the federal-district-court decision in *Bandemer v. Davis* (603 F. Supp. 1479 [S.D. Ind. 1984]) reveals confusion on that court's part on this issue (see Browning and King n.d.). While it may be desirable for the courts to decide on an acceptable range for the type of representation in U.S. states, partisan bias is a separate issue. The courts would obviously prefer a system with no bias, but this too may not be possible. In such a case, the courts might establish an "acceptable" level of bias. Using this method numerous elections over at least a decade are necessary to make a confident determination about the degree of bias and type of representation. Perhaps in those states with a history of bias, the courts might more closely monitor the reapportionment process, or might even direct a court-ordered reapportionment plan. Regardless, this analysis can provide an understanding of the type of democratic representation and the existence of partisan bias over a historical series of elections for a particular state.

Appendix

We show how our emphasis on probabilistic relationships transforms Equation 5. We also resolve a problem existing in the political methodology literature so that β and ρ may be estimated efficiently and consistently. Estimates for each of the states appear in Table A-1.

Several authors have recently attempted to convert the deterministic cube law in Equation 1 (with $\rho = 3$) to a statistical relationship. The main issue here is how to incorporate a disturbance term (Linehan and Schrodt 1978).[25] Schrodt (1981) provides the most comprehensive analysis of possibilities but is ultimately unable to select any particular method: "In the absence of a theoretical justification for a specific error structure in the cube law, there is no *a priori* reason for choosing one . . . model over the others" (Schrodt 1981, 35–36).[26]

For a solution to this disturbance-term problem, we take a different approach. In reformulating the generalized cube law, we find that it is more politically interesting and more natural to model seats (s) than the odds of seats $S/(1 - S)$. The disturbance term should therefore be formulated in terms of the more fundamental and interpretable variable, s. Thus, we can rewrite Equation 5 to include an additive disturbance term:

$$E(s) = D\left\{1 + \exp\left[-\ln\beta - \ln\left(\frac{V}{1-V}\right)\rho\right]\right\}^{-1} \qquad \text{(A-1)}$$

or

$$s = D\left\{1 + \exp\left[-\ln\beta - \rho\ln\left(\frac{V}{1-V}\right)\right]\right\}^{-1} + \epsilon \qquad \text{(A-2)}$$

The expected value operator, $E(\cdot)$, means that the functional form on the right hand side of Equation A-1 (a bilogit function of votes) will correctly predict the number of seats, s, on average over the long run. We assume in Equation A-2 that $E(\epsilon_i) = 0$. This means that, over the long run, the average error is zero.

We now only need to choose a probability distribution for s (or, equivalently, for ϵ). Recall that s is the number of seats allocated to the Democratic party. It is therefore a nonnegative integer ranging from 0 to D, the number of legislative districts. To incorporate as much information into the distribution as possible, we limit the range of possibilities to discrete probability distributions. The distribution should have the mean as a parameter but should not necessarily be symmetric, since the bounds at 0 and D make symmetry either impossible or implausible for means not equal to $D/2$. Instead, a formulation more faithful to the concept of partisan symmetry is required: the distributions with parameters $(D/2) + \lambda$ and $(D/2) - \lambda$ should be mirror images of one another.[27]

The binomial distribution meets each of these requirements and is relatively easy to work with. One possible problem is that district outcomes within a state may not be independent, as is assumed by the binomial distribution. Experiments with alternative formulations that allow dependence among districts were performed and were found to add little to the analyses below. This result is consistent with recent research indicating that congressional elections are local, not national, events, fought primarily within individual election districts (Hinckley 1981). The likelihood equation that emerges from this distribution and Equation 5 is also quite similar to what would result if a Poisson or a variety of other distributions were chosen (see King n.d., 1987); indeed, estimates from this model would be consistent even if the distribution were not binomial but were a member of the family of linear exponential distributions (Gourieroux, Monfort, and Trognon 1984). The results would therefore be quite similar if our assumption were incorrect.

Thus, for the analyses below, s was assumed to be distributed binomially; that is,

$$Pr(s = k; \theta|D) = [D!/(D - k)!k!] \\ (\theta/D)^k[1 - (\theta/D)]^{D-k} \qquad \text{(A-3)}$$

where $\theta = E(s)$, from Equation A-1. D, the number of districts, is assumed to be known a priori. ϵ is defined as $\epsilon = s - \theta$. For a maximum likelihood solution, the log-likelihood equation, reduced to sufficient statistics, can be written by substituting Equation A-1 into Equation A-3, taking logs, simplifying, and summing over all observations:

$$\ln(L_i) = \sum_{i=1}^{n} \{s_i[\ln(D_i) \\ - \ln[1 + \exp(-\ln\beta \\ - \ln\left(\frac{V}{1-V}\right)\rho)]] \\ + (D_i - s_i)\ln[1 - [1 + \exp(-\ln\beta \\ - \ln\left(\frac{V}{1-V}\right)\rho\,\{]^{-1}]\} \qquad \text{(A-4)}$$

The Berndt et al. (1974) numerical-estimation algorithm, in combination with the positive definite secant update method, was used to maximize this likelihood function to derive the estimates described in the next section. Relatively quick convergence was achieved in nearly all cases.[28] The estimates of ρ and β, along with standard errors for each, appear in Table A-1. This table is further discussed and analyzed in the text.

Table A-1. Representation and Bias Coefficient Values

States	$\hat{\rho}$	Standard Error	$\ln(\hat{\beta})$	Standard Error
Alabama	1.95	1.14	-.70	.93
Arizona	1.55	1.78	-.46	.56
Arkansas	1.95	1.10	-.42	1.24
California	.93	.57	.16	.09
Colorado	2.08	2.55	.39	.51
Connecticut	6.11	1.83	.08	.33
Delaware	∞	—	.00	—
Florida	1.68	.97	.51	.44
Georgia	1.64	1.73	.46	1.98
Idaho	6.87	2.90	.14	.77
Illinois	1.34	1.60	-.09	.25
Indiana	5.70	2.39	-.08	.32
Iowa	7.07	2.13	-.31	.41
Kansas	2.43	1.68	-.82	.60
Kentucky	1.16	1.95	.51	.67
Louisiana	2.11	1.40	.04	1.81
Maine	11.87	7.65	-.92	1.50
Maryland	2.35	1.96	.02	.94
Massachusetts	1.96	1.04	-.14	.57
Michigan	3.23	1.96	-.47	.32
Minnesota	1.97	4.62	-.17	.51
Mississippi	1.38	.92	-.04	1.48
Missouri	2.62	2.62	.59	.80
Montana	2.11	4.17	.14	1.38
Nebraska	3.98	2.38	-.82	.93
Nevada	∞	—	.00	—
New Hampshire	6.31	5.48	-.13	1.37
New Jersey	2.83	2.37	.11	.25
New Mexico	5.15	2.34	.25	.44
New York	2.06	.98	.06	.14
North Carolina	∞	—	.00	—
North Dakota	7.43	7.88	-.25	2.20
Ohio	2.04	1.65	-.37	.28
Oklahoma	1.17	5.02	.95	1.58
Oregon	4.74	2.17	-.25	.55
Pennsylvania	1.63	1.25	-.03	.17
Rhode Island	11.23	19.11	-.33	3.59
South Carolina	2.20	1.66	-.12	.75
South Dakota	9.72	6.61	-.30	.94
Tennessee	1.25	1.62	.18	.66
Texas	1.05	.79	1.12	.68
Utah	10.60	4.43	.25	.67
Vermont	∞	—	.00	—
Virginia	2.09	1.54	-.21	.32
Washington	5.39	1.11	-.06	.25
West Virginia	5.67	5.06	.26	1.57
Wisconsin	3.52	1.88	-.32	.37
Wyoming	∞	—	.00	—

Notes

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1. The "swing ratio" is related to what we call *representation*. However, the swing ratio—the slope of an estimated linear relationship between seats and votes—is too restrictive to model the full range of nonlinear representational forms.

2. Because most nondemocratic regimes also have voting, the seats-votes relationship is important there as well. However, the mechanism for translation is obviously very different.

3. In *Davis v. Bandemer* (p. 2809), the Supreme Court recognized that previous cases "clearly foreclose any claim that the Constitution requires proportional representation or that legislatures in reapportioning must draw district lines as near as possible to allocating seats to the contending parties in proportion to what their anticipated statewide vote will be."

4. For most of his analyses, Tufte (1973) used a linear approximation to this form because at the time the logistic form was less commonly used and because the lack of extreme points in his data made the approximation relatively good. Since 1973, the logistic model has been considerably more popular; the figures in this paper will also aid in this interpretation. In addition, the data used in the bias section, as well as the data available from other electoral systems, have substantial numbers of extreme data points. Finally, as the figures will show, a linear approximation is not reasonable for many interesting and empirically common forms of bias and representation.

5. The bilogit form is formally undefined when $\rho = \infty$. As a result, it would be technically more appropriate to characterize the winner-take-all situation as $\rho \rightarrow \infty$.

6. Theil (1969, 521) describes a real electoral situation where the system was created to help the minority at the expense of the majority. Although this antimajoritarian representation system is unlikely to occur much in U.S. politics, it can be represented in the current scheme as $\rho < 1$.

7. In an interesting but wholly impractical proposal, Theil (1969, 524) suggests that citizens express a preference for ρ at the same time as they vote for candidates. Preferences for ρ would then be aggregated before translating votes into seats. Although it is not reasonable to "require the teaching of logarithms at an early stage" (1969, 524), this proposal does suggest the importance of the ρ coefficient in standing for the type of representation in the electoral system.

8. If we used V_R and S_R in place of V and S, respectively, then the bias parameter would be $\beta_R = 1/\beta$. Thus, β is implicitly defined in terms of Democrats.

9. In systems where each party is guaranteed some minimum number of seats regardless of the outcome of the vote, it would be possible to include an additive parameter in Equation 4 to take this into account. However, for congressional elections, this is obviously not relevant.

10. This type of bias occurs, for example, in decisions of legislatures to seat members following closely contested elections. Since the majority party in the legislature can establish its own rules to count disputed ballots, the member seated may not be the candidate with the most votes. In our data, we assume that the candidate with the most votes wins the seat and ignore the bias that occurs if the legislature sat the "loser."

11. These values for β are empirically reasonable in the context of the analysis presented below.

12. Note that this demonstrates that a Gini index measure is inappropriate unless proportional representation is deemed the only fair representation system. If some form of majoritarian representation were considered acceptable, then comparing one of the biased curves in Figure 3 to be the fair proportionality curve in Figure 2 will be misleading; indeed, by that standard, even the fair majoritarian curve would be considered biased. It might be useful to generalize the Gini index and derive a measure of bias based on the area between the unbiased ($\beta = 0$ and ρ unconstrained) and the actual bias (β and ρ unconstrained) curves, thus incorporating all the absolute biases. But this is not needed, since $\ln\beta$ contains all of this information.

13. Indeed, many modern-day physicists do not believe that even the physical world is deterministic (see the discussion in Zellner 1984).

14. Niemi and Fett (1986) review several possibilities and conclude that their "historical swing ratio" is better than their "biyearly form." The main reason is that many data points are better than two. Curiously, however, they conclude their article by favoring the "hypothetical (single-year) swing ratio." They give other reasons for preferring this method, but we do not conclude as they do that many data points are better than two, but that one datum is better than many. Our analysis will therefore use many data points collected over time.

15. The data were split into two nine-election-year samples for each state. The two sets each of β and ρ coefficients were each then correlated. Since positive infinity is a possible value for ρ, a simple measure of association was not possible. Omitting these values usually led to correlation coefficients of about .45. More revealing were scatterplots that indicated closer fits between the two time periods

than any single coefficient could demonstrate. We also split the data to test whether the "one man, one vote" Supreme Court decision had an effect on ρ and β. As one would expect, we concluded that changes in state politics occur slowly. Incumbency, political parties, and geographical and constitutional factors prevent wholesale changes in the structure of representation and bias.

16. One might hypothesize that ρ is a linear function of a vector of explanatory variables, such as state size (X_1) and party competition (X_2): $\rho = \alpha_1 + \alpha_1 X_1 + \alpha_2 + X_2$. It might also be reasonable to hypothesize that β is a function of explanatory variables such as party strength (Z_1) and state ideological orientation (Z_2): $\beta = \gamma_0 + \gamma_1 Z_1 + \gamma_2 Z_2$. The right hand side of these two equations could then be substituted into Equation 5 to derive reduced-form estimates—resulting in a form analogous to interaction effects in regression analysis. This procedure is inapplicable here, however, because β and ρ are assumed to vary only across states. An alternative procedure is used below to express $\hat{\rho}$ and $\hat{\beta}$ as stochastic functions of these explanatory variables.

17. As is apparent from Equation 5, no estimates can be computed when a party received 0 votes for a year or when there is no variation in the number of seats across the years. For this reason, the coefficients of several of the states were not calculated on the full 18 years. The estimates from the following states are therefore not as reliable, since they have one or more years omitted (noted in parentheses): Georgia (1950, 1952, 1958), Louisiana (1950), Missouri (1954, 1956, 1958, 1962), South Carolina (1950, 1958, 1960), and Arkansas (1950, 1954, 1958). This and other causes of imprecision in the estimates are indicated in the standard errors in Table A-1.

18. It is likely that (1) less biased states (i.e., with smaller absolute values of $\ln\beta$) and (2) states where bias and representation vary independently will be closer to consistency than others, but there is no consistent way to know this or to estimate the bias parameter without the full joint estimation presented here.

19. The negative signs indicate bias toward the Republicans; the positive coefficients indicate Democratic bias. Most of the states listed in Table A-1 have estimated ρ and β coefficients within range of what one would expect. The exceptions are mostly those with large standard errors. These substantive points are more directly analyzed in the U.S. House.

20. The residuals from the two regressions correlated at only $-.117$ (t-value $= -.765$). A seemingly unrelated regression would therefore be of no help here.

21. If we let R stand for the raw Ranney index, the measure described as party competition in the text is calculated as $1 - 2\,|R - .5|$. This is the distance from being in between the parties, normalized to a 0–1 scale.

22. For example, the architect of the recent and controversial California redistricting plan, late Congressman Phillip Burton (D–San Francisco), "did not deny that the gerrymander was alive and well in California. Burton publicly joked that his zigzagging district lines were 'our contribution to modern art' " (Lowell and Craigie 1985, 246).

23. In particular, no evidence of regional effects could be found: after controlling for the other variables in the equations, the South and other regions were not more likely to be characterized by different levels of bias or representation. There was also no interactive regional effect on the coefficients from the weighted-least-squares results.

24. The plurality in *Davis v. Bandemer* (n. 9) recognized that there was a range of fair seats-votes relationships that are not proportional. Justices White, Brennan, Marshall, and Blackmun's comment that their opinion "is not a preference for proportionality per se but a preference for a level of parity between votes and representation sufficient to ensure that significant minority voices are heard and that majorities are not consigned to minority status is hardly an illegitimate extrapolation from our general majoritarian ethic and the objective of fair and adequate representation recognized in Reynolds v. Sims."

25. It is useful to express relationships statistically even if there is no explicit sampling procedure. One only need conceptualize the dependent variable (Democratic seats, in this case) as a function of systematic (votes Democratic in the bilogit form) and random factors. The random factors are represented by a disturbance term with some known probability distribution.

26. Schrodt finds that three of the five models "have the undesirable property of varying depending on which party is in the numerator and denominator" (1981, 36). (Our model avoids this problem: ρ is invariant and only the sign of $\ln\beta$ changes by using the Republican, instead of Democratic, party for V.) Of the remaining two, he concludes after empirical analyses that one "appears to have little utility" (p. 41). The final model seems best, but it produces the largest standard errors in applications. It also assumes that the disturbance term is lognormal, which may be appropriate at times, but "is not the sort of probability distribution for an error term that one is likely to choose by default" (Linehan and Schrodt 1978).

27. Most probability distributions are eliminated by these axioms. The Poisson distribution is not bounded from above (King n.d.), and the truncated Poisson distribution does not meet the invariant requirement (Johnson and Kotz 1969). The *beta*-binomial and contagious binomial distributions are possibilities, but the latter makes implausible assumptions about sequential influence structures among congressional districts and the former is elim-

inated on empirical grounds.

28. We estimate lnβ instead of β because it is more easily interpreted. Furthermore, because of the invariance property of maximum-likelihood estimation (DeGroot 1975, 291–92), the exponentiation of the estimate of lnβ may be calculated in order to recover the maximum likelihood estimate of β.

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CONTROVERSIES

MACHIAVELLI'S PARADOX: TRAPPING OR TEACHING THE PRINCE

In The Discourses *Machiavelli extolled the virtues of republican government, yet in* The Prince *he advised the ruler on how to perpetuate autocratic rule. What accounts for this paradox? Mary Dietz argues that Machiavelli sought to deceive the prince, trapping him into actions that would destroy his rule. John Langton contends, in contrast, that Machiavelli was seeking to teach the prince how to govern so that the autocratic state could evolve into a republic.*

In his most substantial work, *The Discourses on the First Ten Books of Titus Livius,* Machiavelli defends the ideals of republican government by arguing on historical grounds that the most stable, solid, and humane states have been founded on social equality, political liberty, rule of law, popular elections, and mixed constitutions embodying a system of checks and balances. Yet in his best known book, *The Prince,* Machiavelli lays out an elaborate, "Machiavellian" program for acquiring and retaining autocratic power and thus seems to encourage a form of practical politics that vitiates the realization of his political values.

Students of political theory have proposed quite a few diverse solutions to this paradox. The prevailing view, it seems clear, is that Machiavelli is actually seeking in *The Prince* to teach an absolute ruler how to use his power to reform a corrupt and feeble state and thereby to lay the foundation for the emergence of a viable republic. In other words, according to the dominant interpretation, Machiavelli regards the absolutism he encourages in *The Prince* as the necessary precondition for the establishment of the kind of republicanism he endorses in *The Discourses.*

Mary Dietz (1986) rejects this and other available attempts to reconcile the politics of *The Prince* and the values of *The Discourses* as unpersuasive. Instead, she advances the contention that *The Prince* is actually a "political act," "an act of deception," a piece of "duplicitous advice," designed to restore a republic in Florence by tricking a "gullible and vainglorious prince," Lorenzo de Medici, into implementing policies that would "jeopardize his power and bring his demise" (p. 781).

This is a novel and provocative thesis, but after analysis and reflection it strikes me as implausible and misleading. It is predicated on a selective, misguided reading of the relevant texts and an inadequate construal of both Machiavelli's fundamental values and his theoretical intentions. In exposing these flaws in Dietz's argument, I want to suggest that a particular "nationalistic" version of the dominant view of the relationship between *The Prince* and *The Discourses* offers a much more tenable interpretation than the one she devises.

Dietz (p. 782) essentially views *The Prince* as a well-disguised trap. The "text itself provides areas of 'solid ground,' or firm advice a new prince . . . can rely upon to gain and maintain power." But hidden amid this solid counsel are " 'ditches and pitfalls' in the form of subversive directives," concealed by "promises of power, glory and popular support." My impression, in contrast, is that *The Prince* is too long, too historical, too involved, indeed altogether too judicious for this to be the case. Putting it another way, why is the solid ground around the putative pitfalls so well prepared—and so extensive?

Machiavelli examines many topics in *The Prince*, particularly in the area of foreign affairs, which have no direct bearing on the problem of maintaining power within a state. And he does so in a rigorous, bureaucratic "issues-and-options" style. Why did he not simply relate the solid counsel in a more straightforward, uncomplicated way? Indeed, if his goal was to bring Lorenzo down, why did Machiavelli not just write a short, snappy, meretricious memorandum on how to govern Florence instead of a densely packed handbook on *realpolitik* (which, by the way, a gullible and vainglorious prince would probably not peruse)? The answer, I think it is clear, is that Machiavelli's purpose was instruction, not deception. Of course, it can be retorted here that the most effective trap always looks like something else, and this explains all the well-tended "firm ground" around the pitfalls in *The Prince*. But are there really consciously prepared pitfalls in the text? Or, to put the question in a more manageable form, Does *The Prince* contain any advice not consonant with what Machiavelli teaches in *The Discourses*?

Dietz contends that Machiavelli puts four main suggestions in *The Prince* that he hopes will induce Lorenzo to dig his own political grave: (1) that he should reside in the city of Florence itself; (2) that he should strive to gain the favor of the people; (3) that he should not build any fortresses; and (4) that he should create a civilian militia or mass-based native army. Let us examine each prescription in turn to determine if it has a sinister intent.

Residing in a Conquered City (Florence)

As Dietz points out, according to Machiavelli, if a new prince wants to effectively govern a republic he has seized or conquered, he should either "destroy it" or "reside in it" (pp. 782–83). In *The Discourses* Machiavelli (1950, 183–84),[1] repeats the advice about destroying conquered cities or provinces in order to rule them securely, but he explains here that the destruction of a society in this context means the reorganization of its government, the redistribution of its population, and the restructuring of its stratification system. In other words, Machiavelli is apparently quite sincere when he advises Lorenzo that he has an option: to solidify his rule in Florence he can destroy the city *or* reside in it. What if Lorenzo had chosen the former option, which Machiavelli himself (1950, 184) describes as "the best means of holding a principality"? Why present an option here, thus giving Lorenzo a chance to follow a path leading away from the pitfall? Indeed, would any of Machiavelli's other alleged pitfalls been effective, if Lorenzo had elected to "destroy" Florence?

But what about the less radical option of residence within the city as a "means of securing possession"? Is this a trap? Dietz construes the suggestion to be a ploy to induce Lorenzo to abandon his country villa and take up residence in the city, where he could be more easily found and destroyed by a vengeful Florentine people still deeply imbued with republican sentiments (p. 783). Now, this argument turns on the assumption that the mass of Florentines had not forgotten "the name of liberty"—that they were, in fact, ardent republicans, just waiting for the opportunity to break the shackles of princely rule. Indeed, Dietz's whole thesis rests on the assumption that "the Florentines had become accustomed to a republic," that "in Florence, the idea of liberty was deeply rooted in political tradition" (p. 784). Yet, she herself quotes (p. 785) a passage from Machiavelli's *History of Florence* where he states that "liberty was unknown" in the city. Moreover, Machiavelli contends in *The Discourses* that republican values, traditions, and institutions were never strongly developed in Florence. The city had its "origin in servitude," and then

> when afterwards the opportunity occurred for her to gain her liberty in a measure, she began by making a constitution that was a mixture of her old and bad institutions with new ones, and consequently could not be good. And thus she has gone on for the two hundred years of which we have any reliable account, without ever having a government that could really be called a republic. . . . And although Florence repeatedly gave ample authority, by public and free suffrage, to a few of her citizens to reform the government, yet these never organized it for the general good, but always with a view of benefiting their own party, which, instead of establishing order in the city, only tended to increase the disorders. (1950, 239–40)

The specific implication of this passage is that Machiavelli clearly does not see Florence as a hotbed of republicanism and certainly did not write *The Prince* under the illusion that most Florentines were chafing to regain their lost liberty. Indeed, all the evidence of *The Discourses* leads to the conclusion that Machiavelli regarded his countrymen as largely "corrupt" and "effeminate" (1950, 284–85, 369, 491). They were not a dangerous, rebellious mass in his estimation and the actual history of Florence after the fall of the republic in 1512 fully confirms this view.

The broader implication of Machiavelli's assessment of his native city is that it explodes Dietz's claim that the purpose of *The Prince* was to "restore" the previous Florentine republic (pp. 781, 794; see also Machiavelli 1950, 111). Machiavelli did not think the old republic was worth restoring: its constitution was not good and its citizens, elites as well as common people, generally lacked civic virtue in the sense that their propensity was to put their private interests before the general good. He wanted the city reorganized, reformed, revitalized, and this, he emphasizes, requires the labor of an absolute ruler (1950, 110–11, 138–39, 166–67d, 170–71). But even that was not sufficient, because from Machiavelli's perspective, no Italian city-state, no matter what form of government it possessed, was really a viable geopolitical entity in a world dominated by large absolutist kingdoms. The only viable entity was the entire country of Italy, united under a strong (ultimately republican) government. As Machiavelli observed at one point in *The Discourses,*

> A country can never be united and happy, except when it obeys wholly one government, whether a republic or a monarchy, as is the case in France and Spain; and the sole cause why Italy is not in the same condition, and is not governed by either one republic or one sovereign, is the Church. . . . The Church, then, not having been powerful enough to be able to master all Italy, nor having permitted any other power to do so, has been the cause why Italy has never been able to unite under one head, but has always remained under a number of princes and lords, which occasion her so many dissensions and so much weakness that she became a prey not only to the powerful barbarians, but of whoever chose to assail her. (1950, 151–52)

Gaining Popular Support

As Dietz relates, there is probably no more frequently repeated piece of advice in *The Prince* than that "the ruler should always strive to gain the favor of the people" (p. 783). Machiavelli not only reiterates this advice again and again but offers apparently prudent suggestions for implementing it (1950, 64–67, 85). How can practical advice about gaining popular support be a trap?

Dietz's conjecture is that Machiavelli's real view of the matter is just the opposite of what he preaches in *The Prince*: in contrast to what he writes there, he actually believes that a dictator in Florence could best secure his position by gaining the support of the nobles, while distrusting and, if necessary, repressing the liberty-living people. In other words, by telling Lorenzo through *The Prince* to be hostile to the nobility and to cultivate the favor of the people, Machiavelli was, in Dietz's view, attempting to trick the Florentine dictator into alienating his natural allies and trusting his natural foes. Dietz supports this conjecture in large measure through a strained interpretation of a memorandum written by Machiavelli to Pope Leo X, entitled "On Reforming the State of Florence."

Yet in *The Discourses* Machiavelli (1950, 162) says that he regards "as unfortunate those princes" who try to control a mass of hostile subjects with "extraordinary measures," such as "cruelty." This only erodes their authority. The "best remedy" for popular hostility is not repression but "to try to secure the good will of the people." And this can be achieved by, on the one hand, massacring the nobles and, on the other hand, allowing the people "to live in security," that is, to conduct government according to the rule of law (1950, 162–63). Whom is Machiavelli trying to deceive here? Although he employs a different argument, he repeats essentially the same advice in *The Prince* (1950, 35–36). Are both books, then, duplicitous? I think not. However Machiavellian their teachings may be, both works are sincere efforts to educate and to exhort.

Building Fortresses

In chapter 20 of *The Prince*, Machiavelli proffers some very equivocal advice about the practice of building fortresses. Indeed, the advice is so wishy-washy that it is difficult to imagine that it would persuade anyone to do anything. Yet Dietz detects a clever trap (pp. 787–88). She thinks this chapter is intended to convince Lorenzo not to build a fortress, because Machiavelli really believes a central citadel would make the dictator less vulnerable to a republican insurrection. But is this, in fact, really Machiavelli's true belief? Dietz herself acknowledges in a footnote that in *The Discourses* Machiavelli unequivocally denounces the practice of erecting fortresses (p. 797). She does not, however, discuss his three main grounds for this advice. First, Machiavelli claims, fortresses are "injurious" because they induce the foolish presumption on a prince's part that he can systematically abuse his subjects and yet control them with force. In the long run this generates tremendous hostility, and no citadel can permanently protect a ruler from an aroused populace. Second, fortresses are "useless" against the artillery of a modern army (1950, 362–68). Finally, and most crucially, fortresses have a debilitating effect on the military capacity of a society: they foster the conviction that a large, well-trained army is unnecessary; whereas, as Machiavelli observes (1950, 369), fortresses cannot preserve states without good armies and they are unnecessary for states with them.

In light of this analysis, I cannot see how Dietz manages to convince herself that Machiavelli's advice on fortresses is a trap. But perhaps he is being duplicitous in *The Discourses* as well when he avers, "A good and wise prince . . . will never build fortresses, so that they may place their reliance upon the good will of their subjects, and not upon the strength of the citadels" (1950, 364).

Creating a Citizen Militia

A new ruler, indeed any wise ruler, Machiavelli strenuously argues in *The Prince*, should always have his subjects armed and trained for combat, and he should never rely on mercenary or auxiliary troops (1950, 44–46, 48, 51–52, 77–78). For Dietz, this piece of advice is perhaps Machiavelli's most treacherous pitfall: "The new prince who arms his subjects may . . . easily make himself a mark for overthrow by creating the very instrument of his own destruction, namely, a civilian militia" (p. 786). Now obviously, arming and training a substantial body of citizens may "facilitate plots, incite insurrection, and inspire rebels." But what leads Dietz to maintain that this is what Machiavelli actually hoped would follow from his advice? She seems to offer two arguments. On the one hand, she intimates that Machiavelli had a high estimation for the republican commitments and revolutionary potential of his countrymen (pp. 786–87); but this, as I have tried to demonstrate, does not appear to be the case. On the other hand, she implies that Machiavelli knew that few successful dictators or princes had actually armed their subjects, and that, in fact, Lorenzo's own grandfather, Lorenzo the Magnificent, had actually disarmed the citizens of Florence in order to secure his dictatorial position. Nothing more graphically reveals Dietz's careless reading of the relevant texts and her misconstrual of Machiavelli's values than this argument.

According to Dietz, "when Machiavelli discusses such virtuous new princes as Francesco Sforza . . . or Cesare Borgia . . . he makes no mention of their having armed their subjects, doubtless because they did not. His bold claim that 'history is full of such examples' [of new princes who armed their subjects] is followed by no examples at all" (p. 786). But in point of fact, Machiavelli cites many examples, including the case of Cesare Borgia himself! As he explicitly points out (1950, 51), Borgia found his mercenary troops "uncertain to handle, unfaithful and dangerous." He therefore "suppressed them and relied on his own men. . . . [Thereafter,] his reputation . . . constantly increased, and he was never so highly esteemed as when every one saw that he was the sole master of his own forces." Perhaps even more damaging to Dietz's argument, in chapter 21 of *The Prince*, a mere three pages after the point where Machiavelli claims that "history is full of examples" of new sovereigns arming their subjects, he discusses the case of his contemporary, "Ferdinand, King of Aragon, the present King of Spain." As Machiavelli observes (1950, 81–82), "Ferdinand . . . may almost be termed a new prince because from a weak king he has become for fame and glory the first king in Christendom, and if you regard his actions you will find them all very great and some of them extraordinary. . . . He was able with the money from the Church and the people to maintain his armies, and by that long war [with the Moors] to lay the foundations for his military power, which afterwards made him famous." For Machiavelli, this is a particularly instructive example, because Ferdinand did for Spain exactly what Machiavelli exhorts some "redeemer" to do for Italy in the famous last chapter of *The Prince*.

Although it now seems almost superfluous to say it, *The Discourses* is actually full of examples of absolute rulers who created popularly based armies—Tullus, Pelopidas, Epaminondas, the "current King of England" (1950, 175–76). In encouraging Lorenzo to establish a civilian militia, Machiavelli was hardly attempting to deceive and ruin the Florentine dictator; instead he was articulating one of his deepest convictions, namely, that "princes and republics of modern times as have no national troops for defense or attack ought well to be ashamed of it; for . . . if there are no soldiers where there are men, it is not owing to any natural or local defect, but solely to the fault of the prince" (1950, 175).

Dietz tries to make her argument about the duplicitous character of Machiavelli's advice by focusing exclusively on questions of domestic politics and internal power. But both *The Prince* and *The Discourses* address as well the other dimension of *realpolitik*, international affairs. And on this subject if there is one message which Machiavelli wants to drive home, it is the utter folly of being without a strong, indigenous army in a world filled with violent and voracious states. Again and again in *The Discourses*, Machiavelli describes Florence as "feeble," and she flounders in this execrable condition precisely because, having few troops of her own, she relies heavily on cowardly mercenaries (1950, 212–14, 285, 317, 369, 385, 491). When Lorenzo the Magnificent disarmed the Florentine people, he committed, from Machiavelli's perspective, a heinous political crime. Creating and maintaining a civilian militia poses risks to a prince's regime, but being without such a force guarantees his country's degradation, subjugation, and inevitable obliteration.

Political Values, Theoretical Intentions

Dietz's thesis rests substantially on the assumption that Machiavelli's only deep values are "republicanism and liberty" (p. 794). But Machiavelli was *also* a passionate nationalist. Indeed, if the last chapter of *The Prince* is a sincere cry for *risorgimento* and not, as Dietz suggests, one more piece of "bait" to trap a vainglorious Lorenzo de Medici (p. 796), then it seems clear that the national unification, security, and glory of Italy were among Machiavelli's most cherished values. The destruction of Lorenzo and the restoration of a small, feeble, corrupt, and badly organized Florentine republic could do nothing for the achievement of these nationalistic aspirations. The establishment of a viable Italian state could, however, set the stage for the reintroduction and evolution of republican institutions. If this was Machiavelli's view and if he shared it with friends and associates, then it is no wonder that he was not given a governmental position in the (ultimately weak and short-lived) Florentine republic that emerged in 1527 after 15 years of Medici rule. The problem was not so much that Machiavelli had sought a position with Lorenzo but that he had become, in terms of his partisan commitments, an Italian nationalist rather than merely a Florentine republican.

In Machiavelli's estimation, the first step in the process of creating a national state in Italy had to be the emergence of some autocratic leader who possessed the knowledge—and the motivation—to accomplish in Italy what Ferdinand had achieved in Spain.

But this is only one aspect of what Machiavelli wants. In *The Discourses* (1950, 116–17, 129–30, 166), he discloses his conviction that the Roman republic stands as the historical model of what a state should be. What Machiavelli ultimately hopes to foster through his writings is the rebirth in Renaissance Italy of the Roman republic in some modernized form. Viewed from this perspective, *The Prince* and *The Discourses* are easily reconciled. After a ruler had carried out the "moral" and practical program outlined in *The Prince*—that is, after he had secured control over a particular city-state, developed a strong, popularly based army, driven "barbarian" forces from Italy, and extended his control over the country—he could turn to *The Discourses*, perhaps near the end of his reign and under Machiavelli's own personal guidance, to learn how to give his new political creation the republican institutions and culture which would make it stable, long-lived and humane, thus ensuring its greatness and the prince's (and Machiavelli's) ultimate glory (see Machiavelli 1950, 145).

Whether Machiavelli actually believed that Lorenzo de Medici was up to this stupendous task, there is no question that he hoped he might be and that he would hire Machiavelli as chief consultant for the project. It is not too farfetched to say that Machiavelli wanted to do for Lorenzo what Count Cavour did for King Victor Emmanuel II in the late 1850s. In sum, Machiavelli was not trying to deceive and ruin Lorenzo; he was trying to educate him about the opportunity for, the demands of, and the glory to be gained from, the unification and political regeneration of Italy. This, of course, is not a novel thesis, but it has the merit of being much more plausible than the argument concocted by Dietz.

In criticizing the kind of reconciliation of *The Prince* and *The Discourses* proposed here, Dietz suggests that Machiavelli was too politically astute to entertain the naive belief that the "heroic politics" of a full-fledged, Machiavellian ruler would "somehow 'give away' to mass politics, that the death of the prince [would] lead to the rise of the republic" (p. 780). But a moment's reflection on what we know about the broad trajectory of European political evolution will show that this view, far from being naive, is incredibly prescient. Absolutism served as the precondition for the establishment of the modern nation-state, which in turn permitted the rise of mass politics. Indeed, this is what eventually happened in Italy. Alas for Machiavelli, he was 350 years ahead of his time.

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Perhaps, as Garret Mattingly once remarked, the puzzle of Machiavelli's *Prince* "has taken up more time and energy than it deserves" (Plumb 1961, 190). Nevertheless, Machiavelli remains a theorist ripe for controversy, and *The Prince* continues to fascinate and invite competing interpretations. In response to my interpretation of *The Prince* as an act of political deception whose advice is intended to trap and destroy Lorenzo de Medici, John Langton resurrects the staple view of the nineteenth century. *The Prince,* he states, is a cry for national self-determination, and Machiavelli himself is an Italian nationalist, rather than an ardent republican. His familiar interpretation rests upon two general claims: first, that *The Prince* is a sincere and straightforward "handbook on *realpolitik,*" written in a "rigorous, bureaucratic, 'issues-and-options' style," and second, that Machiavelli intended the "heroic politics" of the (nationalist) prince would give way (under Machiavelli's guidance) to the republican politics of the *Discourses.*

Since I addressed the second argument at some length in my article and since nothing new has been added to it by Langton, I will not reiterate my criticism of the "from-heroic-to-mass-politics" view here. Suffice it to say that I still find unconvincing the notion that Machiavelli believed that a Medici prince, once secure, would selflessly endow his regime with republican *ordini,* thereby hastening his own political demise. Langton's first claim, however—that *The Prince* contains honest nationalist advice—challenges my interpretation more directly. Let me turn to our particular disagreements over Machiavelli's "advice," after resituating the general problem historically.

Florentine Republicanism

At the center of my interpretation of *The Prince* as an act of political deception stand two key arguments: a historical one, that Florentine republicanism was a living reality when Machiavelli wrote his treatise, and a biographical one, that Machiavelli remained a republican not just from 1498 to 1512 but throughout his life. The latter argument gives him the impetus to plot against the Medici autocrat; the former gives him reason to believe that the political environment would be hospitable to an overthrow if only the conditions to hasten such an event were in place. Langton casts doubt on both of these arguments by contending that after 1512 Florence was hardly a "hotbed of republicanism" and perhaps not accurately viewed as a city deeply rooted in republican traditions at all. Furthermore, Machiavelli himself "did not think that the old republic was worth restoring"; instead, he thought Florence would be better served by an absolute ruler who would "reorganize, reform, and revitalize" it in order to unify Italy. Thus, on two counts—for the city and for Machiavelli himself—republicanism becomes a dead letter, and "nationalism" rises in its stead.

These are indeed bold claims. As Langton notes, by questioning the republican tradition in Florence and Machiavelli's political preferences, he challenges the very context upon which my interpretation depends. But is he correct?

As we know from the discoveries of a generation of scholars, from Hans Baron and J. R. Hale, to Quentin Skinner and J. G. A. Pocock, Florentine thinkers in the fifteenth century developed a political theory celebrating the republican ideals of liberty, civic equality, and an arms-bearing citizenry (Baron 1961; Hale 1977; Pocock 1975; Skinner 1978). At the very least then, Florence seems to have been a "hotbed" of republican ideas. Moreover, thanks to the labors of recent historians, we know that Florence was also a city of strong civic republican practices—of "substantial and lasting" opposition to the Medici stretching back to their first regime in 1434, and of constitutionalist traditions espousing equality before the law, elections by lot, and freedom of speech (Brucker 1969; Hale 1977; Najemy 1982; Rubinstein 1968; Schevill 1936).

Langton comments upon none of this previous scholarship (not even to reject it). What he does declare is that "the actual history of Florence after the fall of the republic fully confirms" that the Florentines were disinclined to attempt to regain their liberty. But to what "actual history" does he refer? The newly restored republic of 1527 may indeed have been "weak and short lived," but it was nevertheless born of a *popular rebellion* against the Medici lords, fully in keeping with a tradition of republican fervor and citizen opposition between 1434 and 1458 and again in 1466, 1478, and 1494, the year of Piero de Medici's overthrow. Hale, Rubinstein, and Schevill ably trace the story of popular unrest in the city after the return of Lorenzo. And if the Florentine propensity to revolt requires any further confirmation, we also have Machiavelli's own words. In advising Pope Leo X on "reforming" Florence, Machiavelli emphasizes the volatility of the city after 1512, warning that

> the whole general mass of Citizens . . . are never satisfied—and whoever believes otherwise is not wise—unless you restore, or promise to restore to them their Authority. . . .
>
> The general mass of Florentine Citizens will never be satisfied except the (Council) Chamber be reopened. . . . Therefore it is the better proceeding that You open it with secure methods and means, and that You take away from whoever was your enemy, the opportunity to reopen it against your will and with the destruction and ruin of your friends. (quoted in Pansini 1969, 635)

The picture Machiavelli paints for Pope Leo reveals a citizenry that cannot cast aside the memory of its ancient liberty. It does not portray, as Langton would have it, a populace immune to the promise of a republic and incapable of reclaiming its republican traditions. What Langton asserts, then, simply flies in the face of historical, textual, and political evidence.

Langton also emphasizes, and with reason, the critical attitude Machiavelli adopted toward his native city as well as his frequent attacks upon its factionalism, its political corruption, and its failure to achieve a stable government akin to the one he so admired in ancient Republican Rome and envied in Venice. Certainly no one, least of all Machiavelli, ever claimed that Florence was the pristine image of republicanism; the city was at best characterized by constant fluctuations between autocratic and republican rule. But do Machiavelli's realist appraisals mean, as Langton claims, that he had no interest in restoring the republic and instead welcomed the arrival of a state-building prince? I find no evidence of it.

In the first place, Machiavelli's criticism may be taken at face value—as evidence of his refusal to bow to idealistic visions and his awareness of what the Florentines were up against with regard to their political survival. Without question, as he acknowledges in his *History of Florence* and in reference to the Pazzi conspiracy, there were times in which "liberty was unknown" in Florence. The period of 1478—when the Medicean reaction against the republican revival of 1466 was in full swing—was certainly one such period. Likewise, Machiavelli was, without question, worried about the (in)ability of those cities "born in servitude" to effect successful republican regimes. But nowhere—including in the passage Langton takes as evidence of Machiavelli's disdain for republicanism in Florence—does he say that Florence would be better off governed by an absolute prince with nationalist aspirations. As Hale has observed, neither Machiavelli nor his contemporaries were asking themselves, "Should Florence be governed by a republican constitution or by an absolute prince?" Rather, Machiavelli's concern was with the question, "What qualities should *our republic* show the outside world, and how can our sick state have its vital tone restored?" (Hale 1961, 181). Thus, Machiavelli does not think, pace Langton, that once destroyed, a republic is best not recovered but rather that once destroyed (or overthrown), a republic faces its greatest difficulty and politics its most important challenge—the restoration of its liberty and civic *virtù*.

As he writes of Rome in the *Discourses*, "in a great republic there are constantly evils occurring requiring remedies which must be efficacious in proportion to the importance of the occasion" (1950, 538). Much the same could be said of Florence, a far less glorious republic to be sure, but a republic nonetheless and in need of "efficacious remedies" to restore it to health. This is the issue that Machiavelli, as political theorist, political actor, and Florentine patriot, faces head on with historical creativity in the *Discourses*, with detailed advice about the restoration of republican *ordini* in "Reforming the State of Florence," and with craft and cunning in *The Prince*.

Pitfalls in The Prince

The force of Langton's criticism turns upon a general thesis about Machiavelli and Florentine republicanism that lacks biographical, historical, and textual support. I will return to this in closing. But now, what of his specific counters to the "pitfalls" I uncover in *The Prince*, especially regarding the Medici residence, the civilian militia, and the building of fortresses? I am afraid that none of Langton's counters succeed, much less do they require abandoning an interpretation of Machiavelli's pieces of advice as traps for the Medici prince.

On the issue of residence, Machiavelli advises Lorenzo either to destroy the city or reside in it. The first choice, far from leading away from the "pitfall," actually draws the prince toward it. The destruction of Florence, as Machiavelli knows, is an outrageous suggestion and a practical impossibility. It renders the truly dangerous second choice—residing in the city—as the only attractive alternative. Thus, Machiavelli would give Lorenzo the illusion of choice even as he narrows the prince's range of possibilities. This is how Machiavelli usually conceives of the complex world of choices and deceptions; consider also Ligurio's ploys in *Mandragola,* where he often plays an outrageous choice off against one that seems reasonable to old Nicia but one which will actually compromise him.

As to the danger of residing in the city, we might recall that the history of republican upheavals in Florence was in part one of the people's taking and retaking the Palazzo Medici, which was vulnerable to mass action in a way the Medici villas in the Tuscan hills were not. Perhaps Machiavelli fondly remembered the ouster of Piero in 1494, when a vengeful Florentine populace drove the family from the palazzo, dragged Donatello's *Judith* from the family gardens, and set it up before the palace of the Signoria with a new inscription warning would-be tyrants and praising civic liberty. Such an event —of practical political as well as symbolic importance—could not have occurred had the Medici prince been fortified beyond the city walls. As Machiavelli understood, to oust a prince a people must have not only the spirit but also the opportunity to get at him.

The matter of Machiavelli's advice concerning "whom to arm" is even more important. I argue that in advising Lorenzo in chapter 20 to "keep his subjects armed" and make "partisans" of them, Machiavelli sets still another trap. He lays the groundwork for a new Florentine civilian militia that could contribute mightily to the destruction of the Medici regime. To convince Lorenzo that new princes regularly arm their subjects, Machiavelli appeals to history but he (uncharacteristically) offers *no* historical examples at all to illustrate the aptness of his advice. Thus, he presents the warrant of history to Lorenzo, but the warrant is in fact a sham, for the advisor knows that the practice he describes as routine for new princes is rare.

Langton, however, claims that Machiavelli offers at least two examples (in chaps. 5 and 21) of new princes who armed their subjects. In Cesare Borgia and Ferdinand of Spain, Machiavelli allegedly provides models whom Lorenzo can use for the course of action outlined in chapter 20. Leaving aside the obvious question —if Borgia and Ferdinand were such good examples, why did Machiavelli not mention them in Chapter 20?—let us turn instead to a more vital question: are Borgia and Ferdinand, in fact, examples of new princes who "always had their subjects armed?" On this score, I would suggest that it is Langton who advances the "careless reading." His misreading hinges on what should be the obvious difference between a "civilian militia" of the kind Machiavelli recommends to Lorenzo, and a "private army" or "national troops" which he credits Borgia and Ferdinand, respectively, with establishing. To put this otherwise, Machiavelli surely appreciates that in the Romagna (the greatest source of mercenaries) Borgia relies upon "his own men" and that in Spain Ferdinand taxed the people to build up his military might. As a result, neither was beholden to foreign soldiers. But nowhere does Machiavelli equate their actions with the creation of an *arms-bearing citizenry.* And it is the latter that he counsels Lorenzo to create in chapter 20, when in essence he advises the rearming of a formerly republican city. Thus, the examples of Borgia and Ferdinand are neither apt nor relevant in this context. In fact, the example of Borgia could be counterproductive, so Machiavelli does not mention him, just as he does not mention *il Magnifico,* Lorenzo's grandfather, who *disarmed* the Florentines and was the most successful of all the Medici lords.

These examples are important. Nevertheless, I think that by seizing upon them, Langton ultimately dodges the most important question of all. Regardless of whether Borgia or Ferdinand fit the bill, is

Machiavelli's advice on arming one's subjects helpful counsel *for a Medici in Florence*? Langton himself concedes that "creating and maintaining a civilian militia poses risks to a prince's regime." Precisely so! But in admitting as much he simply begs the crucial question; he does not resolve it, nor does he persuasively challenge the contextual and historical evidence I present in order to reveal Machiavelli's deception.

To counter my interpretation of Machiavelli's advice on fortresses, Langton turns to the *Discourses,* and notes that there, too, Machiavelli is chary of fortress building and urges that the prince rely instead upon the good will of his subjects. He seems to imply that on matters where there is no contradiction between *The Prince* and Machiavelli's other writings (especially the *Discourses*) there must be no deception at work in *The Prince.* I see no reason to accept such an interpretation, unless one assumes (as I do not) that everything Machiavelli expresses elsewhere can be read as the "truths" that expose the "lies" of *The Prince.* If, however, this is Langton's presumption, he needs to be more consistent in his application of it, and acknowledge not only those passages that seem to point to a sameness in Machiavelli's advice, but also account for those passages where striking contradictions appear. He makes no attempt, for example, to explain the advice against fortress building in *The Prince* in terms of Machiavelli's letter to Guicciardini, where Machiavelli equates the successful Medicean conquest of Florence precisely with the building of a *fortezza.* Nor—to take another example of importance in my essay but unacknowledged by Langton—does he confront the counsel against liberality in *The Prince* with Machiavelli's treatment of it in the *History of Florence* as a valuable Medicean tactic to maintain power. Nor does Langton try to explain how Machiavelli's dictate to Lorenzo to "build upon the people" and be wary of the nobility squares with his straightforward comment to Pope Leo that a prince in Florence "despoiled of Nobility cannot sustain the burden of the Principality," and thus must create a "middle group" between himself and the general public (Pansini 1969, 620). By simply asserting that the latter is a "strained interpretation," Langton again sidesteps the intriguing and difficult issues. He certainly offers no explanation for them.

Pitfalls aside, Langton's interpretation fails finally to confront the methodological premise behind my reading of *The Prince*—that genuinely historical study is the indispensable precondition for interpreting political texts of the past. Other than a few (unsubstantiated) assertions about Florentine republicanism, Langton offers no historical reading of Machiavelli's treatise and uses descriptions the author could not in principle have accepted as his. So, for example, we are given the nineteenth-century language of "nationalist aspirations" or the twentieth-century conception of a "viable geopolitical entity." Furthermore, Langton would have us accept an ahistorical description of *The Prince* and the *Discourses* as works of political literature. As noted, he describes the former as written in a "bureaucratic 'issues-and-options' style," and rhetorically he asks why, if my interpretation is correct, did Machiavelli not simply write a "short, snappy, meretricious memorandum?" But these are anachronisms. The bureaucratic memorandum and the "white paper" were not literary options in the fifteenth century, and within the existing genre of the Mirror of Princes tracts *The Prince* is remarkably short and snappy.

In his conclusion, Langton shifts from an ahistorical to a suprahistorical interpretation of Machiavelli's intentions. He praises the Florentine republican for having the prescience to anticipate nothing less than "the political evolution" of Europe down through the nineteenth century. Among its lesser faults, this credits Machiavelli with a vision of political creation as a linear progress, a view he did not hold nor could have held. To the contrary, his own clearly expressed vision of the cyclical movement of glory, decay, and regeneration, or *anacyclosis,* has more to do with the *revolution* of order and disorder than with the *evolution* of "mass" from "absolutist" politics (D'Amico 1984, 132). Even more troubling, however, is Langton's evident willingness to subscribe to an overly simplified view of European history—only to reconstitute Machiavelli's contribution both to it and to political thought more generally as a grand moment in the telos of the modern nation-state. Writing history backwards, Langton would have us understand Machiavelli as "350 years ahead of his time"! Commentators may well take issue with this or that interpretation of *The Prince,* including the "politics of deception" I find there. But I would like to suggest that we can make headway in our controversies over the meaning of Machiavelli's little treatise only if we return it to the period in which it was written and examine Machiavelli's intentions within his own time, not ahead of it.

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VALUE CHANGE IN INDUSTRIAL SOCIETIES

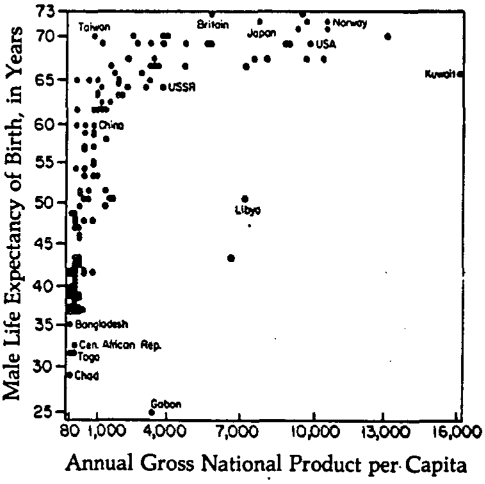
Ronald Inglehart has argued that, while most of the major political parties in Western countries tend to be aligned along a social class-based axis, support for new political movements and new political parties largely reflects the tension between materialist and postmaterialist goals and values. This has presented something of a dilemma to the traditional parties, and helps account for the decline of social-class voting. Scott Flanagan takes issue with Inglehart's interpretation in several particulars. Although their views converge in many respects, Flanagan urges conceptual reorientations and adumbrates a different interpretation of post-World War II political development in Europe and Japan.

Diminishing Marginal Utility at the Societal Level

Ideology lags behind reality. Though Karl Marx died in 1883, his analysis of political conflict continued to fascinate, and sometimes mesmerize, social critics and social scientists for much of the following century. His thesis captured an important part of reality for the early phases of industrial society. But with the evolution of advanced industrial society new conflicts and new world views have emerged, making the economic conflicts Marx emphasized less central to political life.

This development reflects a principle that might be called the diminishing marginal utility of economic determinism: economic factors tend to play a decisive role under conditions of economic scarcity; but as scarcity diminishes, other factors shape society to an increasing degree. In this article we will examine evidence of this phenomenon, first from an aggregate cross-national perspective, then with time-series data at the individual level. The two types of evidence converge, pointing to a diminishing degree of both economic determinism and class-based political conflict as advanced industrial society emerges. Figure 1 provides an illustration.

Figure 1. The Diminishing Marginal Utility of Economic Determinism: 1975 Male Life Expectancy at Birth, by GNP per Capita



Source: Based on data from Charles L. Taylor and David A. Jodice, *World Handbook of Political and Social Indicators* (Cologne: Zentralarchiv fur empirische Sozialforschung, 1982), 3:1948-77.

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As Figure 1 demonstrates, human life expectancy is closely linked to a nation's level of economic development, especially at the low end of the economic continuum: it is virtually impossible for poor nations to attain a high average life span. A large number of countries cluster tightly together at the low end of the spectrum on both income and life expectancy, with such nations as Chad or Bangladesh showing per capita gross national products below one hundred dollars and a male life expectancy of 35 years or less in 1975. Just above them is a group of 20 nations having per capita GNPs of less than three hundred dollars and life expectancies of less than 45 years.

The curve rises steeply with relatively modest increases in wealth, until it reaches about two thousand dollars per capita. Thereafter, the curve levels off. Economic factors become less decisive, and life-style factors more so: longevity becomes less and less a question of adequate nutrition and sanitation facilities, and more and more a question of cholesterol intake, tobacco and alcohol consumption, exercise, levels of stress, and environmental pollution.

The leveling off of the curve does not simply reflect ceiling effects. In 1975 only a few nations had male life expectancies above 70 years; but in the ensuing decade, several nations raised them by several years. By 1982, female life expectancy in Switzerland had risen to 81 years, and this almost certainly does not represent the ultimate biological ceiling. Most developed nations still have considerable room for improvement—but it is no longer so closely tied to economic development. Thus, despite rising income, male life expectancy in the Soviet Union has *declined* in the last decade, apparently as a result of rising alcoholism rates.

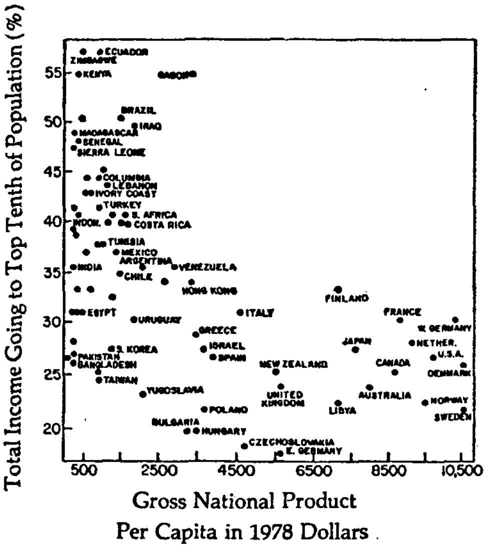
As our hypothesis implies, a logarithmic transformation of GNP per capita shows a much better fit with life expectancy than does a linear model: the former explains 61% of the variance in life expectancy, and the latter only 37%. Figure 1 shows untransformed GNP per capita in order to demonstrate the diminishing impact of economic gains directly. Another illustration of this phenomenon is the fact that among the poorer half of these nations, raw GNP per capita explains 44% of the variance in life expectancy; while among the richer half it accounts for only 14%. Similar patterns of diminishing returns from economic development are found with numerous other indicators of the quality of life. Caloric intake, literacy rates, the number of physicians per capita, and other objective indicators all rise steeply at the low end of the scale but level off among advanced industrial societies.

Equality of income distribution also increases sharply with economic development, up to a level of about 35 hundred dollars per capita; but above that threshold there is virtually no further rise. Figure 2 shows this curve (inverted by comparison with Figure 1, since high scores on the vertical axis represent high levels of *in*equality). In 70% of the nations with a GNP per capita below 35 hundred dollars, the top tenth of the population got more than one-third of the total income (in some cases as much as 57%). In *none* of the nations with a GNP per capita above 35 hundred dollars did the top tenth of the population get more than one-third of the total income; their share ranged from as low as 17% in Communist countries, to a high of 33% in Finland.

Does this cross-sectional pattern reflect a longitudinal trend? The point has been debated. The most reliable longitudinal data come from economically advanced countries which have shown little increase in income equality during the past 30 years. But if we interpret the pattern as reflecting a curve of diminishing returns rather than a linear trend, this finding is exactly what we would expect: it is only in the earlier stages of economic development that we would find large amounts of change. The United States, for example, moved toward substantially greater income equality from 1890 to 1950, but has shown little change since then: absolute levels of income continued to rise, but relative shares changed only slightly. Conversely, Taiwan, South Korea, Singapore and Hong Kong have made a dramatic leap from poverty to prosperity only recently—and all have shown significant increases in income equality (Chen 1979).

Why do we find a curvilinear relationship between economic development and income equality? In the early phase, we believe, it reflects a process of social mobilization, engendered by economic development: industrialization leads to urbanization and mass literacy, which facilitate the organization of labor unions and mass political parties and the enfranchisement of the working class. Economic development does not necessarily have this effect, but it enhances the chances of transforming the masses from isolated and illiterate peasants into organized citizens with the power to bargain for a more equal share of the pie.

Figure 2. Economic Development and Income Equality



Source: Based on data from Charles L. Taylor and David A. Jodice, *World Handbook of Political and Social Indicators* (Cologne: Zentralarchiv fur empirische Sozialforschung, 1982), 3:1948–77.

But why does the curve level off among mature industrial societies? There are two main reasons. First, as one approaches perfect equality, one necessarily reaches a point of diminishing returns. If one reached the point where the top tenth had only 10% of the income, any further transfer of income would be a move *away* from equality. None of these societies has actually reached this point, but some are getting close: in East Germany, the top tenth gets only 17% of the total income, according to official sources (these figures may exaggerate the degree of income equality in Eastern Europe: a Czech analysis cited by Connor [1979, 216–18] estimates that income equality in Eastern Europe is no greater than in some Western countries). In any event, there seems to be a practical limit, for all societies use different rates of pay to motivate their labor force. China, during the Great Cultural Revolution, may have emphasized economic equality more heavily than any other nation in modern times, but some income differences were still retained. To motivate the people, the regime relied on a combination of intense moral exhortation and coercion. This system had severe drawbacks; the current Chinese regime has shifted toward less coercion and more reliance on economic incentives. The fact that Norway, Sweden, and Denmark have significantly greater income equality than the United States, West Germany or France, indicates that the latter countries could move farther toward equality without becoming ineffective economies, or coercive societies. But the Scandinavian countries seem to be approaching the limit of what is possible in a democratic political system.

Why this is so, reflects a second basic principle: political support for increased income equality reaches a point of diminishing returns at a level well short of perfect equality. Przeworski and Wallerstein (1982) have demonstrated through mathematical modeling that under appropriate conditions workers and capitalists would reach a compromise in rational pursuit of their material interests: capitalists would consent to democratic institutions through which workers can effectively press claims for material gains. And workers would consent to profits in the expectation that they will be invested productively, improving their future material gains. Thus, given democratic institutions, some inequality of income may be acceptable to the lower-paid groups.

But as one moves closer to an equal income distribution, the political base of support for further redistribution becomes narrower. In a poor society where the top 10% gets 40%-60% of the total income, the vast majority would benefit from redistribution. In a society in which the top tenth gets only 20%-25% of the total income, far fewer people will benefit from further redistribution, and they will benefit proportionately less; a majority may even stand to lose by additional redistribution more than they would gain. This does not constitute a moral justification for not moving further toward equality; but it does constitute a serious practical problem. Under these conditions, the political base for further development of the welfare state is simply not there, at least not in so far as the citizens are motivated solely by economic self-interest. Ironically, further progress toward equality would come *not* from an emphasis on materialistic class conflict, but through an appeal to the public's sense of justice, social solidarity, and other nonmaterial motivations.

Is there any chance that such appeals would work? We believe that there is. In the long run there seems to be a tendency for the pursuit of economic self-interest itself to reach a point of diminishing returns in advanced industrial societies, and gradually give way to postmaterialist motivation, including greater emphasis on social solidarity (Inglehart 1971, 1977). A large body of survey data, gathered during the past 15 years, suggests that economic development makes a sense of economic deprivation less widespread among mass publics, and consequently, a less important cause of political conflict.

This conclusion is not surprising. After the fact, it may even seem self-evident. It is not: it has been hotly debated and cannot be viewed as conclusively proven even now. A quarter of a century ago, the end-of-ideology school concluded that growing prosperity was giving rise to a *politics of consensus in an age of affluence*; the subsequent explosion of protest in the late 1960s led many to conclude that this school had been completely wrong. In fact, their analysis of what had been happening was partly correct; like Marx, they simply failed to anticipate new developments. While economic cleavages become less intense with rising levels of economic development, they gradually give way to *other* types of conflict.

Thus far we have seen indications that economic development leads to a diminishing impact of economic influences on such objective characteristics as life expectancy and economic equality. But do such changes actually reshape the political preferences of mass publics?

The evidence suggests that at high levels of economic development, public support for the classic economic policies of the Left tends to diminish. Table 1 sums up the responses of eleven Western publics to a set of questions dealing with three key issues underlying the classic Left-Right polarization. These questions were asked in Euro-Barometer surveys carried

Table 1. Support for the Classic Economic Policies of the Left by Level of Economic Development, 1979–1983

		Percentage in Favor of[a]			
Nation (Ranked by per Capita Gross Domestic Product)	1982 GDP per Capita (European Currency Units)	Reducing Income Inequality	More Government Management of the Economy	More Nationalization of Industry	Mean for 3 Issues
1. Greece	3,958	95	82	80	86
2. Ireland	5,408	90	72	64	75
3. Italy	6,287	88	68	36	64
4. Northern Ireland	6,852	76	65	57	66
5. Belgium	8,735	87	53	42	61
6. Great Britain	8,755	73	64	46	61
7. Luxembourg	9,407	82	60	31	58
8. Netherlands	9,830	78	64	32	58
9. France	10,237	93	63	44	67
10. Germany	10,927	80	52	41	58
11. Denmark	11,194	71	57	19	49

Source: Based on combined results from Euro-Barometer nos. 11, 16, and 19; respective *N*s are 8,884 for 1979; 9,909 for 1981; and 9,790 for 1983. See ICPSR codebooks for details concerning fieldwork. Data on GDP per capita are from Eurostat, *Structural Data* (Luxembourg 1985).

[a] Text of questions: "We'd like to hear your views on some important political issues. Could you tell me whether you agree or disagree with each of the following proposals? How strongly do you feel [show card] (1) Greater effort should be made to reduce income inequality. [Those who said they *agree* or *agree strongly* are classified in this table as favorable.] (2) Government should play a greater role in the management of the economy. (3) Public ownership of industry should be expanded. In the 1983 survey, the polarity of Item 2 was reversed: while 70% of the 10 publics agreed that government should play a *greater* role in 1981, only 52% rejected the proposal that government should play a *smaller* role in 1983. For Item 2, these figures are based on the 1981 and 1983 results, only, giving equal weight to the two formulations. Missing data are excluded from percentage base.

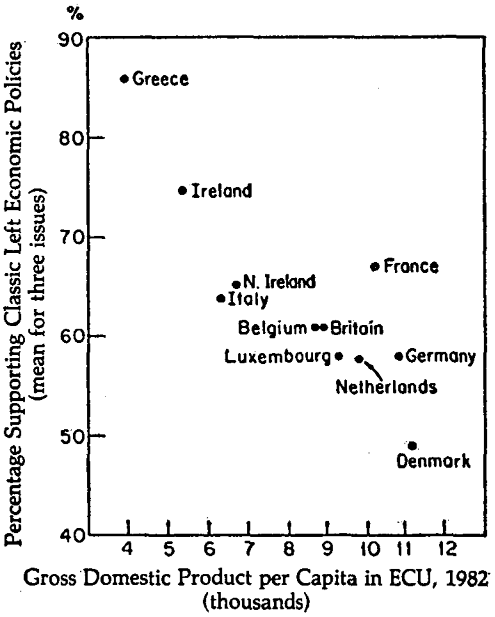
out in 1979, 1981, and 1983 in the 10 member nations of the European Community (Greece, not yet a member, was not surveyed in 1979). The questions deal with redistribution of income, government control of the economy, and nationalization of industry—the central elements of the traditional Left's prescription for society. They were worded as follows:

> We'd like to hear your views on some important political issues. Could you tell me whether you agree or disagree with each of the following proposals? How strongly do you feel? (1) Greater effort should be made to reduce income inequality; (2) Government should play a greater role in the management of the economy [in 1983, this item was reversed, to refer to "a smaller role" and its polarity recoded accordingly]; (3) Public ownership of industry should be expanded.

Though a majority supports greater income equality in every country, while further nationalization of industry is rejected by majorities everywhere except in Greece and Ireland, the *relative* levels of support for these three policies among the publics of given nations show impressive consistency both across items and across time. Taken together, the results add up to a remarkably clear picture of which publics are most favorable to the classic Left economic policies—and the picture does not correspond to conventional stereotypes.

Everyone knows that Denmark is a leading welfare state, with advanced social legislation, progressive taxation, a high level of income equality, and well over half the GNP going to the public sec-

Figure 3. Support for the Classic Economic Policies of the Left by Level of Economic Development, 1979–83



Note: Based on responses to three items, asked in three surveys.

tor. Obviously, the Danish public must be relatively favorable to these traditional policies of the Left. Conversely, everyone knows that Ireland is a largely rural nation, with a modest public sector and no significant Communist or Socialist movements. Clearly, Ireland must be a bastion of conservatism on the classic Left-Right issues.

In fact, the conventional stereotypes are dead wrong on both counts. The stereotypes reflect patterns that were true in the past, but precisely *because* Denmark has now attained high levels of social security—and very high levels of taxation—the Danish public has little desire for further extension of these policies. Instead, support for the classic economic policies of the Left tends to reflect a nation's level of economic development. As Figure 3 demonstrates, Greece is by far the poorest country among the 11 societies surveyed in 1979–83; and the Greek public has by far the highest level of support for nationalization of industry, more government management of the economy, and reducing income inequality. Ireland is the second poorest country, and overall Ireland ranks second in support for these policies. At the opposite end of the spectrum, Denmark is the richest country and has the lowest level of support for these policies. West Germany ranks next to Denmark in economic level and also in support for the classic Left policies.

The French show more support for these policies than their economic level would suggest, which may be linked with the fact that France also has an incongruously high level of economic inequality. But France constitutes the only significant anomaly. The other 10 societies show an almost perfect fit between level of economic development and support for the classic economic policies of the Left.

These findings suggest that the principle of diminishing marginal utility applies at the societal level, as well as the individual level. Greece is an economically underdeveloped country, with many living in extreme poverty and a small affluent elite. In such a context, the balance between rich and poor can be redressed only by strong government intervention. Denmark, on the other hand, is a rich country that has long since developed some of the most advanced social-welfare policies in the world and one of the world's highest rates of taxation. Almost 60% of the GNP is spent by the government, approaching the point where it becomes impossible to move much farther in this direction (even in the Soviet Union, the government's share probably isn't over 75%). In Denmark, further redistribution by the government seems less urgent than in Greece; and the costs of government intervention impinge on a larger share of the population. The incentives to press further with the traditional economic policies of the Left become relatively weak, and public resistance relatively strong.

In this sense, the policies that dominated the agenda of the Left throughout most of this century are running out of steam. Increased state intervention was desperately needed to alleviate starvation and social upheaval in the 1930s; was essential to the emergence of the welfare state in the postwar era; and still makes sense in some areas. But in others, it has passed a point of diminishing returns. The renewed respect for market forces that has emerged throughout most of the industrial world recently reflects this reality. It can be debated whether Gary Hart had come up with the right "new ideas" in 1984, but his recognition of the need for a new liberal agenda was well founded. His rival, Walter Mondale, doggedly but haplessly stuck to the New Deal formula a half century after the New Deal.

The neoconservative claim that the classic welfare state policies have failed is false, however. Quite the contrary: in countries like Denmark these policies have largely solved the problems they are capable of solving and have thereby reduced the demand for more of the same. Insofar as they *succeed*, they reach a point of diminishing returns and begin to cede top priority to problems that have *not* been solved.

Any attempt to turn back the clock to the savage laissez-faire policies of the early twentieth century would be self-defeating, ultimately leading to a resurgence of class conflict in all its former harshness. But the fundamentalists of the Left are equally self-defeating in their rigid adherence to a traditional program based on class conflict and state ownership and control of the means of production.

This does not mean that economic factors are no longer politically important. On the contrary, some of the most significant recent research in political science has demonstrated strong linkages between fluctuations in the economies of Western nations, and support for the incumbent political party (Hibbs, Rivers, and Vasilatos 1982; Kramer 1971; Tufte 1978). But this research has also produced a surprising finding: while support for the incumbents *does* reflect the performance of the national economy, it does *not* seem motivated by individual economic self-interest. The electorates of advanced industrial societies do not seem to be voting their pocketbooks, but instead seem primarily motivated by "sociotropic" concerns: rather than asking "What have you done for me lately?" they ask "What have you done for the *nation* lately?" (Kinder and Kiewiet 1981). Recent research carried out in France, West Germany, Italy, and Great Britain confirms that there—as in the United States—the linkage between the economic cycle and support for the incumbents is overwhelmingly sociotropic (Lewis-Beck 1986).

In short, economics remains an important influence on electoral behavior, but it reflects sociotropic motivations rather than class conflict. The politics of advanced industrial societies no longer polarize primarily on the basis of working class versus middle class; and the old issues, centering on ownership of the means of production, no longer lie at the heart of political polarization.

Political Change at the Individual Level

The argument presented above is implicit in the materialist-postmaterialist value-change thesis; it is new only in its application to the societal level. In this section we examine political change at the individual level. Here we have a rare opportunity: the chance to test a set of predictions about social change that were published years before the data by which they are tested came into existence.

At the individual level, our hypothesis concerning the diminishing role of economic factors, is supplemented by a second basic hypothesis: that early-instilled values tend to persist throughout a given individual's life. In context with the unprecedented economic development of the postwar era, these hypotheses imply a shift from materialist toward postmaterialist values. At the individual level, we should find sizable and persisting differences between the value priorities of young and old, reflecting their differing formative experiences; but at the societal level, this shift will manifest itself only gradually, as one generation replaces another. Moreover, because this shift involves basic goals, it implies a gradual change in the types of issues most central to political conflict and in the types of political movements and parties people support. Finally, it also implies a decline in social-class voting and increasing polarization over noneconomic values (Inglehart 1971, 1977).

The intergenerational value differences these hypotheses predict have now been explored extensively, in 26 different nations. Survey after survey reveals dramatic differences between the goals emphasized by old and young. Moreover, cohort analysis demonstrates that there is no tendency for given birth cohorts to become more materialist as they age, as they would if these differences reflected life-cycle effects. In 1985, given birth cohorts were fully as postmaterialist as they had been 15 years earlier. There were significant short-term fluctuations, reflecting period effects linked with inflation (Inglehart 1985a, 1985b). But by 1985, inflation had subsided almost to the 1970 level. With period effects held constant, there is no sign of the gradual conversion to materialism that would be present if a life-cycle interpretation were applicable. When short-term forces returned to normal, a substantial net shift toward postmaterialism was manifest, most of it due to intergenerational population replacement (Abramson and Inglehart 1986).

The predicted intergenerational value shift seems to be confirmed by a massive amount of empirical evidence, but the predicted changes in prevailing types of political cleavages have barely been touched on. Let us examine the relevant evidence.

From Class-based to Value-based Political Polarization

The idea that politics is a struggle between rich and poor can be traced to Plato. But unquestionably the most influential modern version of this idea has been Marx's argument that throughout industrial society, social class conflict is the central fact of political life, and the key issue underlying the Left-Right polarization is conflict over ownership of the means of production. Marx's influence is reflected not only in a vast literature of social criticism but also in the existence of an entire family of political parties that were inspired by his writings and, in varying degrees, purport to be guided by his analysis. The rise of postmaterialism makes this analysis less adequate today. Let us consider why.

The postmaterialist outlook is linked with one's having spent one's formative years in conditions of economic and physical security. Hence it is more prevalent among the postwar generation than among older cohorts, and tends to be concentrated among the more prosperous strata of any given age group.

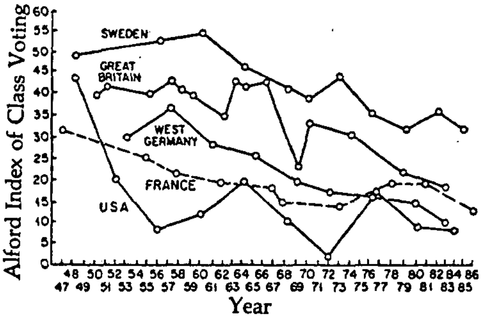
The political implications are significant and at first seem paradoxical. Postmaterialists give top priority to such goals as a sense of community and the nonmaterial quality of life, but they live in societies that have traditionally emphasized economic gains above all, even at the expense of these nonmaterial values. Hence, they tend to be relatively favorable to social change. Though recruited from the higher-income groups that have traditionally supported the parties of the Right, they tend to shift toward the parties of the Left.

Conversely, when postmaterialist issues (such as environmentalism, the women's movement, unilateral disarmament, opposition to nuclear power) become central, they may stimulate a reaction in which part of the working class sides with the Right, to reaffirm the traditional materialist emphasis on economic growth, military security, and domestic order.

The rise of postmaterialist issues, therefore, tends to neutralize political polarization based on social class. Though long-established party loyalties and institutional ties link the working class to the Left and the middle class to the Right, the social basis of *new* support for the parties and policies of the Left tends to come from middle-class sources. But at the same time, the Left parties become vulnerable to a potential split between their postmaterialist Left, which seeks fundamental social change, and their materialist constituency, which tends to take a traditional stance on the new issues raised by postmaterialists.

In 1972, this phenomenon temporarily split the Democratic party in the United States, when an insurgent movement won the presidential nomination for George McGovern. Though he won the postmaterialist vote overwhelmingly, much of the normally Democratic working-class electorate voted Republican, and social-class voting fell almost to zero, as Figure 4 demonstrates. This was an extraordinary election in which nearly half of all Democratic-party identifiers voted for the Republican candidate. In subsequent elections, many Democrats returned to their normal political loyalties, partially restoring class voting. Nevertheless, it now hovers at some of the lowest levels to be found anywhere in the Western world.

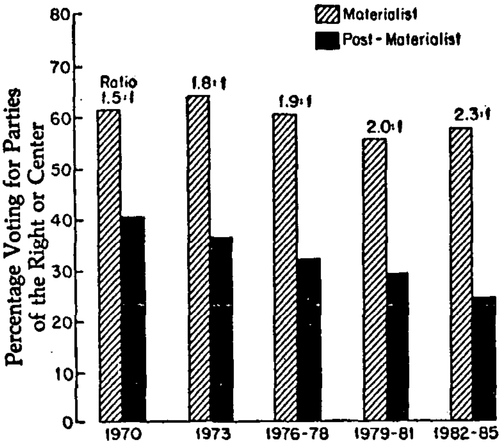
Figure 4. The Trend in Social-Class Voting in Five Western Democracies, 1947–86



Source: Adapted from Lipset 1981, 505; updated by present author with results from recent elections and French elections. British data from Books and Reynolds (1975), Finer (1980); 1983 results calculated from Euro-Barometer no. 19 data; Swedish data, Stephens (1981), Zetterberg (1986); German data, Baker, Dalton and Hildebrandt (1981), Dalton (1984); American data based on whites only, Abramson et al. (1985); French data, MacRae (1967), later data from surveys conducted by Converse and Dupeux (1962), Converse and Pierce (1986) and Euro-Barometer surveys.

In 1981, value cleavages contributed to a more enduring division of the British Left, split between a Labour party that had been captured by a neo-Marxist and neutralist left wing, and a new Social Democratic party that won over much of the party's mass constituency. Throughout the past decade, the materialist-postmaterialist cleavage threatened to split the German Social Democratic party, torn between a postmaterialist "Young Socialist" wing, and the labor-oriented main body. Though the postmaterialist Left was unable to take over the Social Democratic party as long as it still held power in Bonn, they did succeed in launching a Green (Ecologist) party that in 1983 won enough votes to enter the

Figure 5. Percentage Voting for Political Parties of the Right or Center, by Value Type in Britain, France, Italy, West Germany, Belgium and the Netherlands, 1970–85



West German national parliament for the first time.

A longstanding truism of political sociology is that working-class voters tend to support the parties of the Left, and middle-class voters those of the Right, throughout Western society. This was an accurate description of reality a generation ago, but the tendency has been getting steadily weaker. As Figure 4 illustrates, social-class voting has declined markedly during the past few decades. If 75% of the working class voted for the Left, and only 25% of the middle class did so, one would obtain an Alford class-voting index of 50 (the difference between the two figures). This is about where the Swedish electorate was located in 1948, but by 1985 the index had fallen to 31. Norway, Sweden, and Denmark have traditionally manifested the world's highest levels of social-class voting, but all have shown declining levels of social-class voting during the past three decades (Borre 1984, 352). In the United States, Great Britain, France, and West Germany in the late 1940s and early 1950s, working-class voters were more apt to support the Left than were middle-class voters by margins ranging from 30 to 45 percentage points. In the most recent national elections in these countries, this range had shrunk to 8–18 points. In the most recent national elections (from 1983 to 1986) class voting fell to or below the lowest levels ever recorded to date, in Britain, France, Sweden, and West Germany. Though long-established political-party loyalties tend to maintain the traditional pattern, it is being eroded by (1) new support for the Left coming increasingly from middle-class postmaterialists; and (2) by working-class shifts to the Right, in defense of traditional values (Inglehart 1977; Lipset 1981).

It is important to note that the class-conflict model of politics is not a mere straw man: a few decades ago it provided a fairly accurate description of reality. But that reality has changed gradually but pervasively to the point where today, class voting in most democracies is less than half as strong as it was a generation ago. This change seems linked with intergenerational population replacement: throughout Western Europe, social-class voting indexes are about half as large among the postwar birth cohorts as among older groups.

We have argued that Western politics are coming to polarize according to social class less and less, and according to values more and more. We have just seen evidence of the former. Now let us examine evidence of rising polarization according to materialist-postmaterialist values. Figure 5 sums up voting intentions by value type from almost 95 thousand interviews carried out in Britain, France, Germany, Italy, the Netherlands, and Belgium from 1970 to 1985. A vast number of nation-specific events took place in these six nations during 15 turbulent years, which we will not attempt to discuss here. The overall pattern is clear, however: from 1970 to 1985, there was a trend toward increasing polarization on the basis of materialist-postmaterialist values.

In 1970, 61% of the materialists intended to vote for parties of the Right and Center, as compared with 40% of the postmaterialists. Materialists were likelier than postmaterialists to vote for the Right by a ratio of almost exactly 1.5 to 1. This already was a sizable difference, but it has grown steadily larger since 1970. In 1973, the ratio had increased to 1.8 to 1. In 1976–78, it grew to 1.9 to 1. By 1979–81 it was slightly more than 2 to 1. And in 1982–85, the ratio had risen to 2.3 to 1. This changing ratio was mainly due to a loss of postmaterialist votes by the parties of the Right. In 1970, 40% of the postmaterialists supported parties of the Right and Center; in 1982–85, only 25% did so; 75% were voting for the Left.

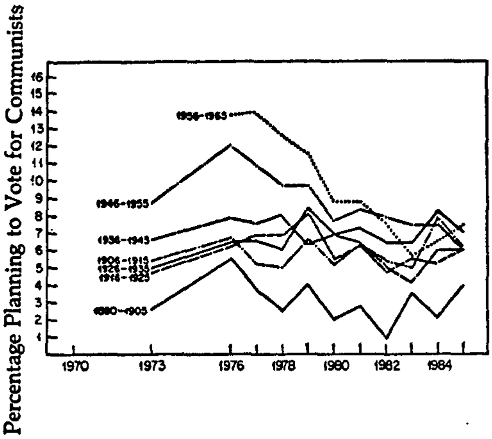
But which Left? In order to fully understand the significance of what has been happening, we must differentiate between various forces *within* a changing and divided Left. There has been only a modest increase in the proportion of postmaterialists voting for the two major long-established parties of the Left, the Socialists and Communists. In 1970, they drew 48% of the postmaterialist vote; in 1982–85, they got 53%. The major gains have been made by New Politics—above all, Ecologist—parties. In 1970 these parties won 13% of the postmaterialist vote. In 1982–85, they obtained 22%. The New Politics gains reflect two countervailing trends: (1) stagnation or decline of the Marxist New Left parties of the 1960s and early seventies; and (2) spectacular growth of Ecology parties, having a distinct and still evolving ideology concerning the quality of the physical and social environment. They have grown from almost nothing in the mid-1970s, to being the largest component of the New Politics parties. In the last few years, Ecology parties have won representation in the national parliaments of Belgium, Luxembourg, and West Germany, and in the delegations to the European Parliament elected in 1984 from Belgium, Germany, Italy, and the Netherlands. Their future potential may be even more than meets the eye, as we will see below.

The rapid growth of the Ecologists and the decline of the New Left parties of the 1960s reflect an important characteristic of both sets of parties: they have not yet developed strong voter loyalties or party organizations. Whether they ever will is an open question. If they don't, in the long run their electorates will probably be absorbed by larger parties that modify their ideological stance sufficiently to present an attractive alternative, just as the Socialist party has absorbed much of the New Left electorate in the Netherlands, partly capturing it, and partly being captured *by* it. We will not attempt to forecast the fate of given parties in given countries; it is influenced by the party's leadership, the strategies they adopt, and by nation-specific events, as well as by the values of the electorate. Figure 5 makes one thing clear, however: since 1970 there has been a growing tendency for electoral behavior to polarize on the basis of the materialist–postmaterialist value cleavage.

Postmaterialists have become increasingly likely to vote for the Left but this trend has become increasingly *selective*, with the Postmaterialist vote going to parties that have distinctive programs tailored to postmaterialist concerns. One striking consequence is that the Communists have lost their relative appeal to postmaterialists. In the early and mid-1970s, postmaterialists were about 2.5 times as likely to vote for the Communists as were materialists. By the mid-1980s, there was little difference between the two groups.

When postmaterialism emerged as a significant political force in the 1960s, its proponents tended to express themselves

Figure 6. Percentage Voting for Communist Party in Nine European Community Nations, by Age Cohort, 1973–85



Sources: Based on representative national samples of publics of Britain, France, West Germany, Italy, Belgium, Netherlands, Luxembourg, Denmark, and Ireland, weighted according to population of given nation. Includes data from Euro-Barometer survey nos. 5 through 23, plus 1973 European Community survey.

in Marxist slogans, which were then the standard rhetoric of protest in Western Europe. To a large extent the term *Left meant* the Marxist parties, and it was natural for the postmaterialists to assume that they were Marxists. But in fact there were profound and fundamental disparities between the goals of the postmaterialists and those of the Marxist Left, as the postmaterialists gradually discovered. These disparities became apparent in France earlier than in other countries—partly because the crisis of May–June 1968 brought to light the basic contradiction between the bureaucratic and authoritarian materialism of the French Communist party, and the postmaterialist desire for a less hierarchical, more human society, in which the quality of life was more important than economic growth. Even there ideological reorientation took many years. But today, a Left has evolved in France that is increasingly non-Marxist or even anti-Marxist, and increasingly independent of the Soviet Union. The French Communist party, on the other hand, has remained one of the most authoritarian and Moscow-oriented parties in the West, with disastrous electoral consequences. After winning 20%–25% of the vote in most French elections from 1945 to 1978, the Communists suffered a sharp decline in the 1980s, falling to 16% of the vote in 1981, then to 11% in 1984, and dropping below 10% in the 1986 election. Though the degree varies from country to country, by the 1980s communism had lost its disproportionate appeal to the growing postmaterialist constituency in Western Europe.

With this has come the decline of the Communists' relative appeal to youth. For decades it had been axiomatic that the young were disproportionately likely to vote Communist: "If a man isn't a Communist when he's 25, he has no heart; if he's still a Communist when he's 45, he has no head." This linkage between youth and the Communist party, however, was not a biological constant; it was based on the belief that the goals of a specific younger generation converged with those of the Communist party.

That belief persisted until recently. As Figure 6 demonstrates, the two postwar cohorts among the West European publics were markedly more likely to support the Communist party than were their elders in each of the Euro-barometer surveys carried out during the 1970s. But the differences were growing weaker in 1978 and 1979. In the early 1980s, Communist support among the two postwar cohorts continued to decline, falling to, or even *below* the level of older cohorts. The young and the postmaterialists had shifted to other parties.

Where did they go? For the most part, *not* to the parties of the Right, despite a good deal of recent talk about the grow-

Table 2. Support for Traditional Left and New Politics Parties by Age Cohort in Britain, France, West Germany, Italy, Denmark, Ireland and Benelux, in 1984–85 (%)

Birth Years	Communists	Socialists and Social Democrats	Ecologists and Other New Politics Parties	Right and Center	Number of Cases
Postwar					
1966–70	5	31	19	45	1,026
1956–65	8	36	17	39	3,782
1946–55	8	40	9	43	3,692
Prewar					
1936–45	7	38	6	50	3,143
1926–35	8	36	4	52	2,810
1916–25	6	40	3	51	2,886
1906–15	7	35	2	56	1,732
Before 1906	3	30	1	66	421

Source: Combined data from Euro-Barometer nos. 21, 22, and 23 (April and November 1984 and April 1985) weighted according to population of each nation (total number reporting a voting intention, 19,492).

ing conservatism of youth. The old Left clearly *is* in decline, which presents an opportunity for enterprising and adaptive parties of the Right and Center. But given the option, younger voters gravitate toward New Politics parties, above all the Ecologists, rather than toward the Right.

As Table 2 demonstrates, support for the Communists no longer bears much relationship to age. The Socialists and Social Democrats have also lost their special appeal for the young in most countries. Nevertheless, support for the parties of the Right and Center is substantially *weaker* among the three postwar cohorts than among all of the older groups. There is no overall trend to the Right. Instead, the slack is taken up by a pronounced rise in support for the Ecologists and other New Politics parties. Consequently, the relative strength of the Communists and the New Politics parties has shifted dramatically among the young. Among the cohorts born before World War II, Communist support outweighs New Politics support by as much as 2:1 or 3:1. In the cohort born in 1946–55, the New Politics parties are slightly stronger than the Communists. And among the two youngest cohorts, support for the New Politics parties outweighs support for the Communists by margins of 2:1 and nearly 4:1.

Neither the young nor the postmaterialists automatically vote for any party that claims to represent the Left. They are influenced by past loyalties, like other voters. But when they abandon these loyalties, they do so in order to support the party that seems most likely to attain their goals, which are not necessarily those of the old Left, and emphatically not those of rigidly authoritarian parties such as the French Communist party. The old Left parties are losing ground among the young: they win the support of only 44% of the 1945–65 cohort, and only 36% of the 1966–70 cohort. But the Right need not win them by default. When an option is available that addresses the postmaterialists' concerns, they tend to take it. The evidence indicates that the Left can win the young *provided* it develops programs that appeal to the postmaterialists, as well as the old Left constituency—clearly not an easy task, but not an impossible one.

Though Western communist parties are in decline, it is extremely unlikely that they will disappear. They still have millions of hard-core loyalists. Moreover, they have made important contributions to politics in their societies: by advocating greater economic equality they stimulated mainstream parties throughout the West to adopt welfare state policies that were motivated, in part, by the need to meet the challenge from the Left, but that helped their societies adapt to the contemporary world.

Today, Marxism itself needs to readapt—and for the sake of survival, is likely to do so. The Italian Communist Party already has reshaped itself extensively. Conservative Marxist parties like the French Communist Party will eventually be forced to do so, regardless of what the current leadership prefers. The need for change is so clear that it is taking place even in societies ruled by communist parties, with first Yugoslavia and Hungary and more recently, the People's Republic of China moving to relax the grip of central controls and allow greater freedom of expression. As the 1980s draw to a close, the Soviet Union itself is experimenting with ways to liberalize its economy and society. Even Marxist movements must respond to the forces of history.

Conclusion

The rise of a new axis of politics, based on polarization between postmaterialist values and traditional cultural values, and the decline of class-based polarization, has left Western political systems in a schizophrenic situation. Most of the major political parties have been aligned along the class-based axis of polarization for decades, and established party loyalties and group ties still hold much of the electorate to this alignment. But the most heated issues today tend to be New Politics issues, on which support for change comes mainly from a postmaterialist middle-class base. This creates a stress that can be resolved in two ways: by a repositioning of the established parties; or by the emergence of new ones. Both have been taking place.

The Marxist interpretation of society seems in decline throughout the industrialized world. Its emphasis on economic factors as the driving force of history provides a good first approximation of reality in the early stages of industrialization, but is of diminishing value as scarcity diminishes and new problems emerge. Similarly, the *policies* that are needed to counter the ruthless exploitation of capitalism in its laissez-faire stage, reach a point of diminishing returns in advanced welfare states. Where government spending is already 40%-60% of GNP, there is less potential to move further in this direction; and the concentration of power in big government itself becomes an increasingly serious problem. The old assumption of the Left that more government was automatically better has lost its credibility. But to elevate government nonintervention into a theological principle is equally untenable. Some of the emerging problems of advanced industrial society will require more government intervention, not less. Within the next few decades, we will need to shift from petroleum to other energy sources or face dislocations far more severe than the OPEC-triggered recessions of the mid-1970s and early 1980s. Developing solar energy and nuclear fusion to meet this need requires a massive research and development effort, decades ahead of time, that market forces are not making and are not likely to make. Governmental inertness in such areas may be disastrous.

The meaning of *Left* and *Right* has been transformed. The key Marxist issue—nationalization of industry—remains a central preoccupation only to Marxist fundamentalists like the embattled hard-liners of the British Labour party. Properly handled, nationalization does little harm; but it is not the panacea it once appeared to be. And in so far as it diverts attention from increasingly pressing problems concerning the nature of modern war and the quality of the physical and social environment, it can be downright counterproductive—for it provides no solution to these problems. Nationalized factories pollute just as much as private ones. Indeed, in so far as nationalization merges the political regulators and the military-industrial complex into one cozy elite, it may even make things worse. Thus, according to a recent UNESCO study, East Germany is the most severely polluted nation in Europe, with air and water pollutant levels two to three times as high as those in West Germany. And it is no coincidence that the only nuclear-power-plant accident that has cost human lives, occurred in the Soviet Union, where public pressures for safety measures are minimized and, until recently, plants were built without containment structures, using a type of graphite reactor that Western countries stopped building decades ago because it was too hazardous. Though their environmental problems are even more severe and their arms expenditures at least as high as those in the West, the political systems of the East European countries rule out the emergence of independent and vigorous environmentalist movements or peace movements comparable to those in the West. It is relatively easy for the ruling elite to simply ignore such issues: officially, the problems underlying them exist only in capitalist countries.

War is as old as human society, but modern technology has given it new implications: today, it could terminate the human race. This fact has shaped a postwar generation for whom war has a different meaning from what it had in previous history. If one still accepts the Marxist conventional wisdom that war is caused by capitalist greed for profits, then one has a ready solution: abolish capitalism. But the fact that most of the bloodiest battles of the past decade have been fought *between* nominally Communist countries—with Cambodia against Vietnam, Vietnam against China, China against the Soviet Union, and the Soviet Union against Afghanistan—has led to growing skepticism that the answer is this simple. The problem is rooted in the mentality of a military-industrial complex deeply rooted on *both* sides of an obsolescent ideological boundary.

The old ideological paradigm no longer corresponds to reality. Neither the Marxist fundamentalists nor the laissez-faire fundamentalists have adequate answers for the problems of advanced industrial society. The goals of individuals and the challenges facing society are different from those of a generation ago.

RONALD INGLEHART

University of Michigan

This controversy harks back to a debate I started with Ronald Inglehart in 1979 (Flanagan 1979), which has found its clearest expression to date in an exchange of articles in 1982 (Flanagan 1982a, 1982b; Inglehart 1982). There are two reasons for revisiting this debate now. First, Inglehart's most recent contribution to the values debate, presented above and in a companion piece,[1] represents a significant shift in his position, which I believe brings his argument much closer to my theoretical view. Second, new data is now available to present a definitive test, at least in the Japanese context, of the issues that divide us. Thus, I believe we are finally in a position to take a giant step towards a resolution of the debate.

The crux of the debate revolves around my argument that there are two distinct kinds of value change taking place in the advanced industrial democracies and that Inglehart has obscured this distinction by collapsing indicators of both into a single scale. My argument has been that these

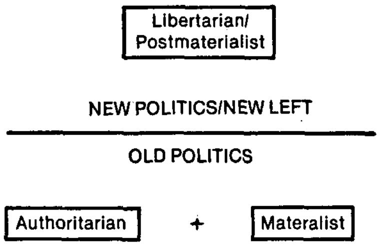


Figure 7. Inglehart's View of Cleavage Structures in Advanced Industrial Democracies

two kinds of change are not only conceptually distinct but are explained by different causal phenomenon and exhibit different patterns of relationships with key demographic and political variables.

Libertarian-Postmaterialist Values

The distinctions between our respective positions can be confusing because we use overlapping but not identical kinds of indicators to measure our respective scales, and similar labels to identify different conceptual phenomenon. To help clarify the differences, Figures 7 and 8 depict our contrasting conceptualization of value change, with the types of indicator items used to measure the scales presented in boxes. Each line represents the axis of a value cleavage, with labels on either side of the cleavage indicating the political impact of the particular type of value orientation defined by the cleavage.

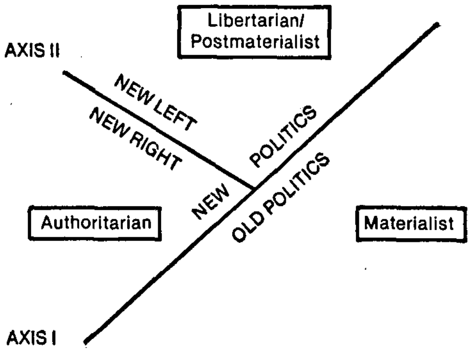


Figure 8. Flanagan's View of Cleavage Structures in Advanced Industrial Democracies

First it should be noted that our three scales, my two and Inglehart's one, are measured by essentially only three kinds of items. What I call *libertarian* and Inglehart and others label *postmaterialist* items are essentially identical. Looking across the full collection of items that have been designated by these labels, we find they include an emphasis on personal and political freedom, participation (more *say* in government, in one's community, and on the job), equality, tolerance of minorities and those holding different opinions, openness to new ideas and new life styles, environmental protection and concern over quality-of-life issues, self-indulgence, and self-actualization (Calista 1984; Hildebrandt and Dalton 1978; Inglehart 1977; Lafferty and Knutsen 1985). There may be some minor differences over which of the above elements are more or less central to these two concepts, but to the extent that they are all found to cluster, I expect none of the above authors would have trouble accepting any of these elements as part of the concept of *postmaterialism.* I include them all in my notion of *libertarianism,* so we have two different labels representing the same cluster of items. While I believe that Inglehart's term *postmaterialism* is a misnomer as a characterization of either the causes or nature of this value cluster, I will label it here *libertarian-postmaterialist* to clarify that regardless of how we choose to identify these items, we are measuring essentially the same set of values.

The differences emerge at the other end of our scales. I have used the term *materialism* in a more limited sense than Inglehart to identify the emphasis attached to economic concerns, both for oneself and one's society. In my sense of the term, then, *materialists* are those who place a high priority on a stable economy, economic growth, fighting rising prices and, at the more personal level (the private domain), on securing a high-paying job, adequate housing, and a comfortable life. Inglehart includes in his *materialism* concept, however, a second set of noneconomic issues, namely support for a strong defense, law and order, and fighting crime. I label this second set of noneconomic issues as one component of an *authoritarian* value orientation. This *authoritarian* orientation designates a broader cluster of values, which, along with concerns for security and order, includes respect for authority, discipline and dutifulness, patriotism and intolerance for minorities, conformity to customs, and support for traditional religious and moral values. To avoid confusion, I will use the term *materialism* in this discussion to refer to my definition of the term rather than his.

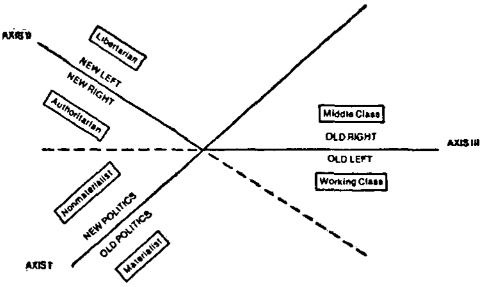
As Figure 7 shows, Inglehart in his original argument presented us with one dimension of value change. Those holding his combination of what I label *materialist* and *authoritarian* value orientations defined the Old Politics for him, while those with the libertarian-postmaterialist orientation the *New Politics* or *New Left*, terms which he used virtually synonymously. He made no mention of a New Right.

This conceptualization encounters difficulty when it attempts to explain realignment. On the one hand we can readily grasp why the younger generation of highly educated, middle-class respondents, whose families have traditionally supported the Right for economic reasons, may now be induced to vote Left as a result of their socialization into libertarian-postmaterialist values. Inglehart's argument breaks down in trying to explain why working-class voters should abandon their historic support of the parties of the Left. Here his argument has rested on a variation of the *embourgeoisement* thesis that he still endorses (Inglehart and Rabier 1986, 458). In his original formulation he wrote that working-class respondents who "have attained a certain level of prosperity relatively recently" will "continue to place a comparatively high value on defending and extending their recent gains" (Inglehart 1971, 992). Because the value priorities of these working-class respondents remained primarily acquisitive or materialist, their growing share of the good life would lead them to become more economically conservative and, hence, potential recruits for the conservative parties.

However, while the working-class Tory phenomenon is well documented, it is capable of explaining the behavior of only a deviant portion of the working class (Goldthorpe et al. 1968; Hamilton 1967). Conservative parties do not defend the economic interests of the working class, and certainly we cannot expect a major realignment of working-class support from Left to Right based on the economic appeals of conservative parties. Moreover, realignment via *embourgeoisement* applies only to the most affluent portion of the working class. However, since affluence in Inglehart's formulation is the primary cause of postmaterialism, these are the very workers most likely to be developing postmaterialist values, which should only reinforce their traditional support for the Left.

In contrast to Inglehart's view, my conceptualization defines two distinct dimensions along which values are changing. The first value cleavage, running along the lower-left-to-upper-right diagonal, taps the priority a respondent attaches to economic issues as opposed to noneconomic, value issues. This cleavage divides materialists from nonmaterialists and the Old Politics from the New Politics. Since materialists attach primary importance to economic concerns, they

Figure 9. Full Cleavage Structure for Advanced Industrial Democracies



tend to relate to politics in economic terms, which corresponds to the traditional class politics of the Old Right and the Old Left party alignments. Conversely, the nonmaterialists emphasize the importance of value concerns over economics. This emphasis on value issues defines the New Politics and includes, for example, *both* prochoice and right-to-life alternatives and *both* strong defense and antinuclear positions. Nonmaterialists, then, are those holding either authoritarian or libertarian value preferences, and those who place a higher priority on the kinds of issues defined by these value preferences than on economic issues.

The New Politics is divided by the second value cleavage, which distinguishes the New Left from the New Right. Those falling on the libertarian side of this second value cleavage support for the New Left issue agenda, including liberalizing abortion, women's lib, gay rights, and other new morality issues; protecting the environment, antinuclear weapons, and other quality-of-life issues; and support for protest activities, more direct forms of participation, and minority rights. On the other side of this value cleavage, the authoritarians endorse the New Right issue agenda, which includes right-to-life, anti-women's lib, creationism, antipornography, and support for traditional moral and religious values; a strong defense, patriotism, law and order, opposition to immigration and minority rights, and respect for the traditional symbols and offices of authority.

Figures 7 and 8 are presented to demonstrate the contrasting ways that Inglehart and I have chosen to combine three types of items into our respective scales. Figure 9 depicts the full cleavage structure and its implications for realignment. As in Figure 8, Axis 1 again divides the Old Politics from the New Politics and materialists from nonmaterialists on the basis of the relative salience accorded to economic, as opposed to value, issues. The relevant cleavage on the New Politics side of Axis 1 is Axis 2, which divides the New Left from the New Right and libertarians from authoritarians. The relevant cleavage on the Old Politics side of Axis 1 is Axis 3, which divides the Old Right from the Old Left and the middle class from the working class. As the dotted extension of the New Left-New Right value cleavage into the Old Politics domain in Figure 9 suggests, the Axis 1 value priority distinction and the Axis 2 value preference cleavage are essentially independent of each other. Thus we should expect to find libertarian-materialists as well as authoritarian-materialists. The line is dotted in the Old Politics domain because for the materialist, who places greatest priority on economic concerns, the libertarian-authoritarian distinction will have little effect on his or her voting behavior. Similarly, the Axis 3 class cleavage extends as a dotted line into the New Politics domain, but for nonmaterialists these class distinctions will not be paramount in their voting decisions.

It is important, therefore, to reach an independent determination of a respondent's position on both value dimensions. An authoritarian-libertarian value preferences scale will tell us whether the respondent is likely to support the New Right or New Left issue agenda. A materialist-nonmaterialist value priorities scale will tell us whether these New Politics kinds of value concerns or the Old Politics economic issues will be foremost in the voter's mind when he or she makes a choice. This latter distinction is particularly important for predicting the behavior of cross-pressured voters, who, for example, may fall on the Left side on the Old Politics cleavage because of their working-class occupations and union memberships but on the Right side of the New Politics cleavage because of their authoritarian values.

The conceptualizations of value change in Figures 8 and 9 can explain the realignment of working-class voters to the Right more effectively than can Inglehart's conceptualization (Figure 7). Across Axis 2 in Figure 9 there has been a long-term shift from authoritarian to libertarian values associated with the changing circumstances under which younger generations are being socialized and the growth of higher education. As the number of libertarians in the advanced industrial democracies reached a critical mass, they began pushing for New Left issues and achieving some successes. The increasing articulation of libertarian values and the protest movements organized to press for the adoption of the New Left agenda mobilized a backlash among authoritarians, who felt that their basic values and way of life were being threatened. This increasing polarization on the New Politics issues in turn heightened their salience in relation to the Old Politics class issues, which were already on the decline due to growing affluence and the success of the welfare state. Thus the shift across Axis 2 from a heavily asymmetric balance in favor of the traditional authoritarian values to a more symmetric balance between authoritarian and libertarian values heightened the salience of New Politics relative to Old Politics issues, thereby inducing movement across Axis I as well. Since education is related to the class and values cleavages in a cross-cutting pattern, associating high education with both the Old Right and the New Left, the combined trends on both dimensions promote the middle-class-to-the-Left and working-class-to-the-Right realignment pattern Inglehart and others have been describing.

One thing that is new in Inglehart's most recent contributions is that he is now beginning to adopt much of the terminology of the Figure 9 conceptualization explicitly. However, he is doing this without in any way revising or altering his scale, which collapses the two dimensions into one and hence treats authoritarian values and the New Right issue agenda as essentially synonymous with materialism. This approach of grafting a new terminology onto his old conceptualization simply adds to the confusion.

Examples of this subtle shift in terminology are abundant in his recent discussions of the value change-realignment phenomenon. In this discussion, Inglehart first reiterates the argument that he and others have made explaining why the New Politics values cleavage is an important phenomenon to study even though there is as yet little evidence that it has induced a realignment of party systems in the advanced industrial democracies. That argument points out that the expression of the New Politics value cleavage in patterns of party support is inhibited by frozen party loyalties and institutionalized interest-group-to-party linkages (Dalton, Flanagan, and Beck 1984; Flanagan and Dalton 1984). Nevertheless the rising salience of the New Politics is increasingly placing these party systems under stress. Perhaps Inglehart's most important contribution in this regard is to demonstrate that the meaning of *Left* and *Right* is changing among the elites and mass electorates of these societies from economic-issue orientations to noneconomic-value-issue orientations (Inglehart 1984). And most recently, he demonstrates that the amount of class-party realignment that can be shown to have already taken place is mostly benefiting New Left and New Right parties rather than the traditional Old Left and Old Right parties and that beyond that, there exists considerable potential support in these electorates for New Left and New Right kinds of parties (Inglehart and Rabier 1987; and the opening to this *Controversy*).

In this discussion of the shift from the Old Politics to the New Politics and its implications for realignment, he seems to buy into many of the distinctions presented in the conceptualizations of Figures 8 and 9. He says that the New Politics issue polarization "primarily reflects the new noneconomic issues" and that "the issues that define Left and Right for Western publics today are not class conflict [issues], so much as a polarization between the goals emphasized by postmaterialists, and the *traditional social and religious values* emphasized by materialists" (Inglehart and Rabier 1986, 470, 471; emphasis added). While still using the term *materialists*, the cluster of values and issues preferences that emerge from his own factor analyses reported in his 1984 and coauthored 1986 pieces as being associated with self-placement on the Right end of a Left-Right scale are that the military-defense effort should be stronger, the existing social order should be defended, terrorism should be punished more severely, nuclear power plants are essential, the unemployed don't want to work, there are too many immigrant workers, one should sacrifice oneself for one's country, abortion is wrong, religion is important, and God exists. To apply the label *Materialist* to these New Right issues is clearly a misnomer. Moreover, the New Left issues he identifies (proenvironment, abortion, peace, homosexual rights, etc.) clearly did not "stimulate a materialist reaction" in the sense of an increased concern for economic issues. Quite to the contrary, the New Left issues have helped to crowd the economic issues off the agenda and have rather provoked the emergence of the above New Right set of moral and religious issues.

Here we begin to see the problems inherent in the theoretical arguments and conceptual labels that Inglehart has attached to his value scale. The emergence in his own studies of a New Right cluster of noneconomic value issues around the New Politics cleavage that fit my definition of authoritarian values implies a delinking of the New Right with the concept of *materialism.* The point is that the New Right is as much *nonmaterialist* as the New Left. The program of the National Front, the French example of a New Right party, stresses law and order, restrictions on immigration, opposition to abortion and anticommunism. It is because these issues have little or nothing to do with traditional class issues and material, economic concerns that the New Right parties are able to draw their support disproportionately from the working class, as Inglehart so clearly demonstrates.

A New Theoretical Basis for Materialism

In my view, the most important fresh contribution found in Inglehart's analysis presented above is the new theoretical basis he provides for understanding the shift from materialist to nonmaterialist issue priorities. He labels this argument "the diminishing marginal utility of economic determinism." The diminishing-marginal-utility approach is a much sounder theoretical foundation for explaining the shift from economic to noneconomic value priorities than Maslow's need hierarchy. Maslow presents us with a theory of psychological development and motivation arguing that an individual reared in an environment in which lower-level needs are satisfied will develop into a mature personality, one who as an adult is better able to cope with the deprivation of lower-level needs and hence is insulated from need regression (Maslow 1970). In contrast, Inglehart's items typically tap issue priorities in the domain of public policy. He is, then, dealing with a vastly different phenomenon, and thus the inappropriateness of the Maslowian argument leaves him with no rationale for explaining why respondents would not increasingly place a higher priority on the issue of rising prices when inflation becomes an increasingly serious problem in their country. Indeed the evidence suggests that they will (Flanagan 1982b).

What the *diminishing marginal utility* concept suggests, however, is something quite different than the Maslowian early-childhood-socialization notion. As Inglehart's Table 1 and Figure 3 demonstrate, the higher a nation's GNP per capita, the weaker both the felt need for further reducing income inequality and the other classic economic policies of the Left designed to redistribute the wealth more equally. As Inglehart argues, after a certain level of equality has been achieved, further movement in the direction of perfect equality has a diminishing marginal utility, as there is less and less left to redistribute, and fewer gain and more lose in the process.

Figure 1 in his companion piece (Inglehart and Rabier 1987), most clearly presents the lesson that is to be learned from the diminishing-marginal-utility thesis. As median income goes up in a nation, the ratio of what the respondent views would be the absolutely necessary income to what the actual household income is drops. In other words, more people find themselves with a comfortable surplus. Their basic economic needs are being met, so economic issues become less intensely held and are assigned a relatively lower priority than they were when household income was at or below the level of absolutely necessary income. All other things being equal, the proportion of nonmaterialists will rise as average household income within a nation rises above a level perceived as necessary to provide the basic necessities of life. As the margin of surplus income increases, the citizen's expenditure of energy in support of economic issues will yield a diminishing marginal utility, and the nonmaterialist is born.

Being a nonmaterialist at time one, however, does not ensure that a respondent will still assign a relatively low priority to economic concerns at time two. As has been shown elsewhere, the relative priority that a respondent attaches to economic issues in relation to other concerns varies across the life cycle as the respondent's economic responsibilities and burdens change (Flanagan 1982a; Milkis and Baldino 1978). A respondent's priorities will also shift in response to short-term changes in perceptions of how well-off he or she is. Aggregate stability in the proportion of materialists, therefore, is a function of national affluence and will remain rather stable so long as economic conditions in the country do not change. Nevertheless, aggregate stability can mask considerable instability at the individual level.

Inglehart claims that his diminishing-marginal-utility (DMU) argument is not new but was implicit in his original value-change thesis. Rather, the logic of the DMU argument as a causal explanation of value change is completely different from his original Maslowian approach. It should be recalled that it was his reference to Maslow's hierarchy of needs that enabled Inglehart to claim that the materialism-postmaterialism value change was the product of early childhood socialization. The Maslowian analogy gave birth to the idea that socialization under conditions of affluence and security create a distinct prioritization of needs that immunizes the individual from regression to lower-level concerns as an adult, even if the conditions of life change. Apparently Inglehart has found that there is a diminishing marginal utility in citing Maslow for his purposes, and indeed we find no references whatsoever to Maslow or the need hierarchy in either of his two current companion pieces. I believe history will show that his Maslowian argument was a clever analogy that did not fit.

In contrast, childhood socialization logically plays no role in the DMU argument. An estimation of the diminishing marginal utility of added increments of income is a rational-choice assessment based on the individual's current level of need and sense of relative deprivation. This assessment, then, is very much a *context-dependent* phenomenon, very similar to the life-context arguments that Milkis and Baldino (1978) and I have made. What is new in Inglehart's notion is that he has changed the relevant context from the individual level, a level at which we would expect to find considerable change across the life cycle, to the societal level. In doing so, he has convincingly made the case that we should find important thresholds in national income and household income, which, once crossed, yield aggregate shifts in the relative priority attached to economic concerns. These thresholds are probably not absolute but rather are likely to shift somewhat in line with changing societal standard-of-living expectations. Still, the notion that at some point a sufficient surplus should lower the priority attached to economic issues and to further increasing one's income, not in every case but in the aggregate, is compelling.

The *diminishing marginal utility* notion provides some legitimacy for using the term *postmaterialist.* I have preferred the term *nonmaterialist* because of my focus on the fluctuation that occurs on the individual level across the life cycle. However, the notion that advanced industrial societies as a whole are moving towards a lower priority ranking for materialistic concerns suggests more of a unidirectional shift, perhaps justifying the *post-* prefix. Still this is clearly not the kind of irreversible phenomenon as suggested by the Maslowian arguments. Indeed we would expect that serious economic problems, such as runaway inflation or depression, would alter the current assessments of many respondents as to the relative priority that should be attached to economic concerns.

Thus, we are dealing with two distinct kinds of value change explained by very different kinds of causal phenomenon. On the one hand we are witnessing an eclipse of the salience of economic issues. This trend is explained by the diminishing-marginal-utility argument. As the affluence of, and equality within, nations increases, the percentage of the population that enjoys a cushion of surplus income above what is needed for the basic necessities of life grows. For these people, noneconomic issues begin to gain in salience relative to material, economic concerns. There may be some variation in the priority accorded to economic issues at the individual level over time, with changes in fortune and life context, and also short-term setbacks at the aggregate level associated with economic downturns. Over the long haul, however, we should be seeing a net growth in the proportions of *postmaterialists*—as here defined in terms of the relative priority attached to economic issues (Old Politics) as opposed to noneconomic, value issues (New Politics).

The second and distinct trend is the one that defines the change in value preferences. Within the so-called New Politics can be detected a long-term, gradual movement from the New Right to the New Left poles, as a function of age (intergenerational change) and education. Space constraints will not permit a full elaboration here of the causal mechanisms, elaborated elsewhere, behind this pattern of value change (see Flanagan 1979, 1982a, 1984). In brief, the argument is that an intergenerational pattern of value change along the authoritarian-libertarian (A/L) dimension has resulted from four major changes in the basic conditions of life under which successive generations have been socialized—a growing equality in incomes and life styles, the accelerating pace of change, the advance and diffusion of scientific knowledge, and the rise of the "no-risk" society (Aharoni 1981). These changes are increasingly liberating mankind respectively from the constraining conditions of subservience to authority figures, conformity, ignorance, and insecurity, and enabling the individual to pursue more fully the goal of self-actualization. Given the nature of many of these changes in basic life conditions, which are driving the change along the A/L value dimension, it is not difficult to understand why the increasing levels of higher education found in the advanced industrial societies are also playing an important role in diffusing libertarian values.

New Evidence of Dimensional Independence

At this point one might raise the question why—if I am in fact right—it has been so difficult to demonstrate the problems with Inglehart's scale empirically and why he has been able to pile up evidence from an exhaustive number of surveys conducted in 10–15 nations over the past 15 years. For example, Inglehart has consistently claimed that there is no evidence of a life-cycle effect on the proportion of materialist found in given age cohorts over time and that there was no substantial reduction in the proportion of his postmaterialists across a large number of countries despite inflation, recession, and other economic woes in the seventies. Moreover, he seems to have the evidence to make good on those claims.

Such a finding is central to his argument because it supports his notion that the materialist-postmaterialist change is a function of early learning that insulates the individual against later change. However the problem here is that while Inglehart labels his scale *materialist-postmaterialist*, 75% of the items used to operationalize the scale rather tap the authoritarian-libertarian dimension. This latter dimension, which measures how respondents divide on the New Politics value cleavage, is not affected by changes in the economy. Changes in one's income, all other things being equal, will not affect one's position on abortion, pornography, or nuclear weapons or one's respect for authority. The relative aggregate stability of Inglehart's scale over time in the face of changes in the economic context, therefore, is derived in large part from the fact that his scale, in essence, is tapping more of the New Left-New Right (libertarian-authoritarian) cleavage than the New Politics-Old Politics (materialist-nonmaterialist) division.

What further confounds our ability to disaggregate the two dimensions combined within his scale is the format of his value-priority questions, which artificially forces an association between his materialist and authoritarian items. Whether using his original 4-item format or his expanded 12-item version, respondents are typically presented with groups of four alternatives and asked to select their first and second priorities. In each case two items are—to use my terminology—libertarian (e.g., free speech and participation); one item is authoritarian (e.g., defense); and one is materialist (e.g., rising prices). This presents the respondent with a *dilemma of constrained choice*. Libertarians who are also nonmaterialists will naturally select the two libertarian items. Libertarians who are materialists, placing greater priority on their economic than on their value concerns, will pick the one materialist item and then, having no other materialist item to pick, will select one of the libertarian alternatives, dropping them into Inglehart's mixed category. Authoritarian-materialists, following the same logic, will pick the materialist item first and then select the authoritarian option.

The real problem, however, is confronted by the authoritarian-nonmaterialists. As a nonmaterialist the respondent will be mostly concerned with the noneconomic, value issues. But he or she finds only one authoritarian option. As an authoritarian, the respondent is very unlikely to select either of the libertarian options and thus, by default, is most likely to pick up the economic option as a second choice. The result is that the authoritarian-nonmaterialists are classified as *materialists*, which is an incorrect coding for a materialist-nonmaterialist scale but a correct coding if we view Inglehart's scale as a stand-in for the authoritarian-libertarian scale.

This logic explains two important characteristics of Inglehart's scale. First it helps us understand why we always find a heavily skewed distribution with many materialists and typically only 5%-15% postmaterialists, while the authoritarian-libertarian type of scale tends to divide samples much more evenly. Although both types of nonmaterialists are properly sorted, the authoritarian-materialists are classified as *materialists* while the libertarian-materialists are classified as mixed. Secondly, and more importantly, whatever real association there may be between authoritarian and materialist orientations is greatly exaggerated by the constrained-choice feature, which forces authoritarian-nonmaterialists to select the materialist option.

The answer to this problem is to adopt an item format that allows an independent and unconstrained assessment of the priority of all three types of items identified in Figure 7—libertarian, authoritarian, and materialist. This is a little tricky, due to the inherent differences between value priorities and value preferences. The materialism items are designed to tap the relative importance of economic and value concerns, both of which are presumably positively valued by the respondent. In contrast, authoritarian and libertarian values stand in opposition to each other, with those who stand at one pole typically viewing the opposite set of preferences in a negative light.

The solution to these problems is to follow the approach developed for the 1976 German Youth Survey conducted by the United States Information Agency (see Dalton 1981). Respondents are handed a card with a number of social and personal goals listed and in each case are asked to indicate whether they feel that their society places too much or too little emphasis on that goal. For the value-priority items, this allows for a rough ranking of the items' importance; for the value-preference items it indicates on what side of the New Politics value cleavage the respondent is found. Since all three types of items are asked in the same way, this procedure will enable us to determine how they cluster in an unconstrained situation. If Inglehart is correct, the materialist items on economic concerns should cluster with the security subset of authoritarian items, and the rest of the authoritarian items should lie distinctly outside of that cluster. If I am correct, all the authoritarian items should cluster together while the materialist items form a distinct separate cluster. A third option somewhere in the middle would be to find that all the authoritarian and materialist items cluster together.

The data to test these conflicting hypotheses comes from a 16-item value question I designed using the above unconstrained format administered in a 1984 nationwide Japanese survey of university-educated respondents.[2] Due to the richness of the value themes tapped by the survey, it was possible to add several more items, bringing the total to 21, with 9 libertarian, 8 authoritarian and 4 materialist items. As shown in Table 3, the *libertarian* items cover the themes of participation, support for minorities, open-mindedness, personal freedom and freedom of speech, improving the quality of life, self-fulfillment, and self-indulgence. The *authoritarian* items include the themes of patriotism, a strong defense, respect for authority and strong leaders, preserving traditional morals and values, conformity, and intolerance of dissenters. The *materialist* priorities were measured by the four items identifying those who place high emphasis on maintaining high economic growth, securing a high-paying job, and working hard and saving for the

Table 3. Factor Loadings for 21 Libertarian, Authoritarian, and Materialist Items on the First Unrotated Factor and for the Three-Factor Rotated Solution, Japan 1984

	Factor Loadings For			
		Three-Factor Rotated Solution		
Items by Type	1st Unrotated Factor	1st	2d	3d
Libertarian items				
Personal freedom	.611	.523	−.293	.267
Freedom of speech	.530	.413	−.292	.279
Giving people more say in government decisions	.475	.548	−.117	−.098
Increasing benefits for disadvantaged	.475	.672	.032	−.047
Improving environment and quality of life	.436	.633	.073	.113
Active citizen participation in local politics	.363	.318	−.185	.040
Being open-minded to new ideas	.309	.421	.015	.040
Seeking personal fulfillment	.309	?.286	−.081	.395
Living for today and enjoying oneself	.281	?.188	−.143	.431
Materialist items				
Securing a high-paying job	.104	.131	.100	.664
Working hard and saving for the future	.002	.299	.390	? .329
Maintaining high economic growth	−.114	−.116	.132	.541
Feel our society is not too materialistic	−.115	−.192	.037	.482
Authoritarian items				
Preserving traditional morals and values	−.197	.150	.468	−.009
Following custom and neighbors' expectations	−.325	.066	.592	.145
A few strong leaders better than parties	−.343	−.131	.380	.044
Respect for authority	−.351	−.106	.419	.029
Providing for strong defense forces	−.491	−.349	.399	.295
No room in Japan for dissenters	−.509	−.103	.633	−.145
Patriotism and loyalty most important	−.631	−.268	.638	−.106
Should increase defense spending	−.637	−.468	.456	.106

Note: The three-factor solution was derived from a varimax rotation.

Table 4. The Association among the Libertarian, Authoritarian, and Materialist Item Scales and between Those Scales and Several Key Demographic and Attitudinal Variables

Scales and Variables	Libertarian	Authoritarian	Combined Authoritarian-Libertarian	Materialist
Authoritarian scale	-.32			
Materialism scale	.13	.23	(-.07)	
Age	-.28	.30	-.37	(-.02)
Right-Left party identification	.28	-.39	.41	(-.04)
Right-Left self-placement	.30	-.41	.44	(-.05)

Note: All correlations are significant at the .001 level except those in parentheses, which are not significant even at the .01 level.

future and those that do not feel that their society overemphasizes material things.

As Table 3 shows, the libertarian items all load positively on the first unrotated factor and the authoritarian items all load negatively, while the four materialist items all fall in the middle, closer to a zero loading. Even more dramatically, the table demonstrates that with the exception of two libertarian items and one materialist item that have somewhat ambiguous loadings, the rotated three-factor solution neatly divides the three types of items, with the libertarian items loading on the first factor, the authoritarian items on the second factor, and the materialist items on the third. In discussing the exceptions below, it is important to note that all of the items included in each of the three reported value domains drawn in Table 3 load highly on a first unrotated factor when the analysis is limited to only those items within a given value type. The inclusion of these three items (marked *?*) within their identified value domain is only brought into question because of the presence of other factors on which they also load.

Each type of item was then combined into a scale and the resulting three scales were then correlated with each other and with several key demographic and attitudinal items as shown in Table 4. As expected, a strong inverse correlation was found between the libertarian and authoritarian scales (−.32). Moreover, the libertarian scale is inversely associated with age (−.28) and positively correlated with the Left ends of the Right-Left self-placement and party identification scales (.30 and .28 respectively). Conversely, the authoritarian scale is associated with old age (.30) and the political Right (−.41 and −.39). The strength of these correlations is somewhat further increased when all the libertarian and authoritarian items are combined into a single scale. In marked contrast, the materialism scale yields no significant correlations with either age or political preference. Unfortunately, the relationships with the other key demographic variable, education, could not be tested because the sample was limited to the university educated. However, the relationships reported in Table 4 are precisely those reported in my earlier study (Flanagan 1982a), only now the materialism scale is derived from the kinds of materialism items Inglehart employs (rather than on a post hoc basis from a most-important-problem question), so there can be no question of comparability. The earlier study included education and class, and we can infer that the relationships would be the same here if the sample had not been restricted on those dimensions.

The most important finding in Table 3 is that the security items Inglehart uses to measure materialism cluster with the authoritarian items while the economic items stand alone. The most important finding in Table 4 is that the pattern of relationships supports my argument that the materialism-nonmaterialism and authoritarian-libertarian dimensions of change are explained by different causal phenomenon and yield contrasting patterns or relationships with key demographic and political variables.

The most interesting unexpected finding in Table 4 is that materialism is positively correlated not only with the authoritarian scale (.23) but also with the libertarian scale (.13). Since I have viewed materialism as essentially an utilitarian orientation, I expected some significant correlation between the authoritarian and materialist items based on the following logic: The relative importance a respondent will place on the economic issues of the day—inflation, depression, unemployment, further economic growth, etc.—is a function of both the respondent's margin-of-income surplus and the extent to which the respondent perceives that his or her economic interests are being threatened by those issues. Thus new middle-class professionals in the advanced industrial democracies are likely to be nonmaterialists, both because of the substantial cushion their incomes provide them with beyond the basic necessities of life and because the skills they possess are highly valued in postindustrial economies, providing them with great job security. Conversely, the blue-collar worker is more likely to be a materialist, both because of his or her smaller income surplus and because this respondent's skills are increasingly becoming obsolete, thus heightening concern over livelihood.

The finding of positive correlations with both of these opposite value preferences suggests that those lying towards both of the extreme ends of the authoritarian-libertarian value dimension might have other kinds of inducements beyond utilitarian evaluations. It may be that something in the values themselves that are held by the extreme form, or pure type, of authoritarian or libertarian predispose each of them towards materialist priorities. Clues as to what these motivations might be are derived from the three items in Table 3 with ambiguous loadings. The materialism item *working hard and saving for the future* actually loads slightly more heavily on the authoritarian dimensions than on the materialism dimension. Elsewhere I have argued that the values of frugality, discipline, and hard work are part of the traditional authoritarian orientation (Flanagan 1979).

One might just mark this down as a bad item, since it combines the authoritarian orientation towards diligence and the traditional "Confucian" and "protestant" work ethics with the materialist concern with economic well-being. If we look more deeply, however, we may find a *psychological materialism*; that is, the authoritarian's preoccupation with discipline and hard work, in contrast with the self-indulgent libertarian orientations, which place a high priority on leisure activities, may be related to the same heightened need for power and sense of weakness that drive one to identify with established authority figures and symbols. Thus the same insecurities that prompt one to support a strong military and law and order may also motivate hard work, frugality, and a heightened effort to maximize one's economic security to placate anxieties about one's own weaknesses and lack of power. Psychological studies have provided evidence suggesting that such authoritarian needs for power and feelings of weakness and insecurity are the product of child-rearing practices that stress discipline over affective ties, a behavioral distinction that has been shown to correlate with class and education (Adorno et al. 1950; Winter 1973). Thus, a number of lower-class, low-education authoritarians may also be psychological materialists.

Just as revealing is the loading of the two libertarian items, *seeking personal fulfillment* and *living for today and enjoying oneself,* on the materialism factor. This suggests a Yuppie type of *terminal materialism.* Some libertarians who are preoccupied with self-fulfillment and self-indulgence may place an excessive valuation on maintaining a strong economy, acquiring high-paying jobs and surrounding themselves with all the material trappings of affluence. While for some libertarians, materialism may only be a secondary instrumental priority, something to be valued because it makes other life goals possible; for others, materialism may become an end in itself—a terminal value—because of the status, self-esteem, sense of achievement, comfort, self-indulgence and other gratifications that it provides. Some libertarians, then, may be susceptible to a "terminal materialism" in which, notwithstanding their relative affluence, increasing their wealth, possessions, and consumption of the good life comes to be valued more highly than other kinds of libertarian values. Thus, for them, economic issues may take precedence over their support for the New Left issue agenda.

The diminishing-marginal-utility thesis is a powerful one for explaining why there is a higher proportion of nonmaterialists in the United States than in Colombia. However, after reaching and surpassing some level of societal and personal affluence, further gains in affluence will have little effect on altering one's value priorities, and we are left with having to explain why some upper-middle-class respondents are still materialists. Indeed there is some evidence of a countertrend in the United States running against the logic of the diminishing-marginal-utility thesis. The American Council on Education's (1973–86) annual nationwide surveys of roughly two hundred thousand entering college freshmen, which began 20 years ago, have documented a growing emphasis on materialism. In 1966 only 45% of the respondents nationwide selected *money: being well off* as an essential or very important life goal. By 1976 this had increased to 52% and by 1986 to 74%. Undoubtedly for some of these entering freshmen, money is viewed as a strictly secondary instrumental priority. On the other hand, it is likely to be a vital priority for others and the notion of a Yuppie type of terminal materialism may be useful here in explaining the failure of materialism to wither away among some of the affluent.

As shown in Figures 10–11, when the three items with ambiguous loadings are dropped and the remaining 18 items are factored again, the factor plots yield three tightly clustered and distinct value domains. The plot of the first and second factors in Figure 10 depicts a sharp differentiation of the libertarian and authoritarian items along both dimensions while the materialism items fall near the origins of both axes. The plot of the second and third factors show the libertarian and authoritarian items splitting on the second factor but lying close to the origins on the third, while the materialist items load heavily on the third factor but lie close to the origins on the second.

When the value scales are recomputed based on the smaller set of 18 items, the correlation between the libertarian and materialism scales drops from .13 to an insignificant but still positive .04. The correlation between the authoritarian and materialism scales also drops from .23 to .18, and if the two authoritarian items loading below .40 are removed, the correlation declines further to .14. The finding of a higher correlation between authoritarianism and materialism in the

Figure 10. Two-Dimensional Plot of the Item Factor Loadings for the 18-Item Three-Factor Varimax Rotated Solution (horizontal axis = factor 1; vertical axis = factor 2)

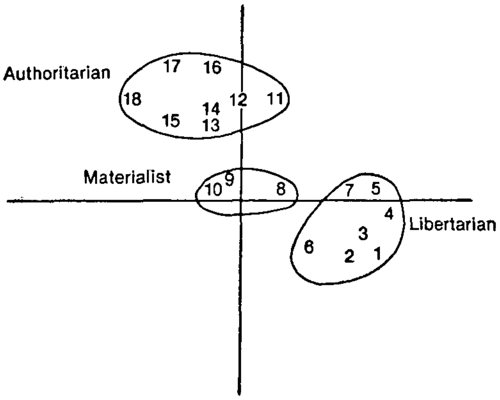
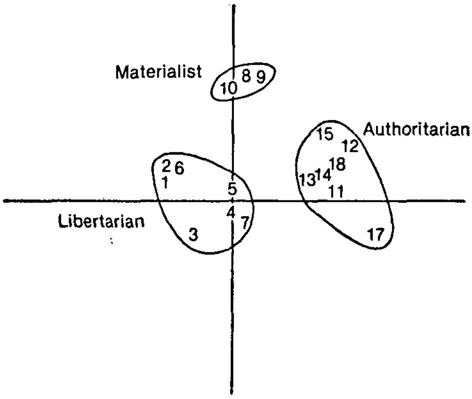


Figure 11. Two-Dimensional Plot of the Item Factor Loadings for the 18-Item Three-Factor Varimax Rotated Solution (horizontal axis = factor 2; vertical axis = factor 3)



Note: Items 1-7 are respectively the first seven libertarian items listed in Table 3, Items 8-10 are the three materialist items (omitting *working hard*), and Items 11-18 are in order of the Table 3 listing of the eight authoritarian items.

case of a class-constrained sample of university-educated respondents, where the utilitarian argument discussed above is likely to have less relevance, suggests that even among the highly educated there are still more authoritarians who are psychological materialists than there are libertarians who are terminal materialists.

Where Do We Go from Here?

Inglehart's theory of postmaterialist value change and his related research clearly constitutes one of the most important theoretical contributions in the comparative field over the last two decades. My argument here and elsewhere has been that Inglehart was correct in identifying an important value cleavage with strong relationships to vote choice and important implications for realignment. Where we differ is in my claim that he has incorrectly conceptualized and measured the value cleavage that divides the New Left and New Right. To date, Inglehart has resisted tampering with his scale because it "works." As I have shown, however, it continues to work for the wrong reasons —namely that it is rather tapping the authoritarian-libertarian dimension while at the same time causing a forced constraint between the authoritarian and materialist items, making it very difficult to disentangle the two distinct value dimensions using his procedures.

The conceptualization of the politically salient value cleavage that divides the advanced industrial democracies has been dominated by the notion of *materialism* for too long. As the wide variety of conceptual subdimensions associated with the authoritarian-libertarian scale suggests, we now need to cast our nets more broadly to determine what clusters with what, and in this regard much work remains to be done. While the results reported above appear to provide definitive proof for my arguments, some cautions are in order. A sample that cut across all levels of education might have yielded somewhat higher correlations between the authoritarian and materialism scales, due to the utilitarian argument. Also, in other countries cultural differences might somewhat affect what does and does not cluster within the authoritarian and libertarian factor spaces. Notwithstanding these caveats, the above evidence makes a strong case for now moving beyond Inglehart's original scale and developing two distinct scales. The two-scale approach is superior because it enables us to distinguish between the materialist-nonmaterialist value-priority dimension, which simply identifies whether the Old Politics economic issues or New Politics value issues are of higher salience to the respondent, and the authoritarian-libertarian dimension, which identifies whether the respondent is likely to support the New Left or New Right issue agendas.

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Notes

1. My response to Inglehart's new contribution grew out of my review of a conference paper he presented at the IPSA conference in Paris in July 1985, which in published form was divided into two articles, including the above offering and a second one published elsewhere (Inglehart and Rabier 1986). My comments, therefore, will occasionally refer to this second companion piece as well.

2. This Japan's Successor Generation survey was conducted under the sponsorship of the United States Information Agency in October and November of 1984, yielding 803 completed questionnaires of university-educated Japanese between the ages of 20 and 91. I am greatly indebted to the principal investigator of the study, James S. Marshall of the East Asia and Pacific Branch of the USIA's Office of Research, for including my values battery in the survey and making the data available to me (see Marshall 1985).

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RESEARCH NOTE

SOPHISTICATED SINCERITY: VOTING OVER ENDOGENOUS AGENDAS

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The empirical findings on whether or not legislators vote strategically are mixed. This is at least partly due to the fact that to establish any hypothesis on strategic voting, legislators' preferences need to be known, and these are typically private data. I show that under complete information, if decision making is by the amendment procedure and if the agenda is set endogenously, then sophisticated (strategic) voting over the resulting agenda is observationally equivalent *to sincere voting. The voting* strategies, *however, are sophisticated. This fact has direct implications for empirical work on sophisticated voting.*

Over the past decade, considerable theoretical attention has been devoted to voting in committees. The focus has been on two related questions. First, given an arbitrary agenda, what is the structure and consequence of strategic (sophisticated) voting and how does this differ from sincere (naive) voting? Second, given strategic voting and an arbitrary status quo, what final outcomes can a monopoly agenda setter reach by an appropriate choice of agenda? For decision making under the amendment procedure[1] with complete information, the answers to these questions are now known (see, in particular, Farquharson 1969, McKelvey and Niemi 1978, and Moulin 1979 on the first problem; and Banks 1985, Miller 1980, and Shepsle and Weingast 1984 on the second).

In the wake of the theoretical advances, empirical work on legislators' voting behavior has become increasingly directed toward trying to discover whether legislators do in fact vote strategically (Bjurulf and Niemi 1978; Denzau, Riker, and Shepsle 1985; Enelow 1981; Enelow and Koehler 1980; Krehbiel and Rivers 1985). For if they do, then any legislator's voting behavior cannot be used unequivocally as an instrument to reveal his or her (legislative) preferences. The findings are mixed: this is at least partly because, as Krehbiel and Rivers remark, "to assess the voting strategies of [legislators] one needs to know [their] preferences" (1985, 3), and preferences are private information. Bjurulf and Niemi and Enelow (and Koehler) find evidence of strategic voting; Denzau, Riker, and Shepsle examine a case in which some legislators appear to vote strategically and others do not; and Krehbiel and Rivers are unable to reject the hypothesis of sincere voting.

To account for legislators not voting strategically to promote their prima facie interests, Denzau, Riker, and Shepsle extend the domain of individual preferences to "home-style" constituency concerns. With this wider framework, they show that there are situations in which it

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is rational not to vote strategically: the cost of doing so in terms of the probability of reelection is too high. Because preferences and motives are private information, constituents are unable to disentangle the reasons why a legislator voted in any particular way. Since they can never be sure whether their representative was voting strategically or simply against their interests, the legislator faces a risk, when deciding to vote strategically, that constituents will believe the legislator is voting against their interests. The evidence that "home-style" considerations are important in voting is strong (Kingdon 1973). But the Denzau, Riker, and Shepsle explanation is problematic in that it makes it even more difficult to test any hypothesis of strategic voting. If a legislator is observed to vote sincerely when there exists an opportunity to be strategic, it is prima facie impossible to reject the hypothesis of sophisticated behavior (relative to the broader domain of preferences): the unobserved "home-style" costs may be too high.

Krehbiel and Rivers (1985) estimate legislators' preferences and find that sophisticated and sincere voting coincide for the issue they study. Consequently, "failure to reject sincere voting does not automatically imply rejection of sophisticated voting" (p. 26). They conjecture that, since the issue was one-dimensional and a median most-preferred outcome existed, a process of amendments and counteramendments resulted in this median being proposed. Although the existence of a median on the agenda does not, in and of itself, guarantee that sincere and sophisticated voting will coincide, the suggestion is that the locus of strategic behavior may be more at the agenda-setting stage of the process than at the subsequent voting stage.

I show that this suggestion generalizes. If decision making is by the amendment procedure, if the agenda is set endogenously, and if information is complete, then sophisticated voting over the resulting agenda is observationally equivalent to sincere voting. The voting *strategies* that support the observed voting behavior, however, are sophisticated in the classical sense. The intuition behind the result is straightforward. Given an arbitrary agenda, a legislator knows that rational committee members will vote strategically. So the legislator takes these voting strategies into account when selecting a proposal or amendment to put on the agenda: proposing is "sophisticated." And given the structure of sophisticated voting on binary agendas, this results in choosing proposals and amendments that defeat previously proposed alternatives under sincere voting. The class of agenda-setting procedures explicitly covered by the result is quite broad. Effectively, any mechanism satisfying the following three criteria fall within it: (1) legislators are given at most one opportunity to offer an alternative; (2) proposals are made openly within the committee; and (3) the agenda is set sequentially, that is, the s^{th} proposal on the agenda can be offered only after the first $(s - 1)$ proposals have been fixed and before the $(s + 1)$-to-final proposals are volunteered.

In the next section, the model and the result, which is technically quite trivial, are developed formally. A final section offers some concluding remarks.

Model and Result

The model is fairly standard. A committee N consists of an odd number, $n \geqslant 3$, of individuals, $N = \{1, 2, \ldots, n\}$. Each individual $i \in N$ has preferences defined on the k-dimensional issue space, R^k: i's preferences are assumed representable by a continuous and strictly quasi-concave utility function, u_i: $\mathrm{R}^k \rightarrow \mathrm{R}$. The committee makes a choice from the alternative space, X ($\mathrm{R}^k \supseteq X$), using simple majority voting over an agenda based on the amendment procedure. An agenda of length t is a t-tuple $\mathbf{y} = (y_1, y_2, \ldots, t_t)$ $\in X^t$. The agenda is *based on the amendment procedure* if the sequence of votes over the agenda is given by first putting y_t against y_{t-1}; putting the winner of this contest against y_{t-2}; and so on until y_1 is paired against the winning alternative in the $(t - 2)$th contest: the winner of this last contest is the *final outcome*. Let $\Sigma(\mathbf{y})$ be the voting tree defined by this process. Hereafter, leave the dependency of $\Sigma(\bullet)$ on $\mathbf{y}$ implicit, and write $\Sigma(\mathbf{y}) \equiv \Sigma$.

The agenda is determined endogenously. The mechanism assumed is as follows.[2] If there is a historically given status quo, labeled $b(0)$, then this occupies the position of y_1 in any agenda. Thereafter, alternatives are offered by eligible committee members in a sequence, π. Although all committee members have voting rights, it is not assumed that all committee members are necessarily eligible to make proposals: agenda setting may be confined to a subcommittee. Let M, $N \supseteq M$, be the set of legislators eligible to offer proposals and amendments. Let $|M| = m$.

The first legislator under π is chosen by some (possibly degenerate) lottery over M. Let $p(j = i_1)$ be the probability in this lottery that legislator $j \in M$ is the first-ranked individual in π. Assume $p(j = i_1) \in [0, 1]$, $\forall j \in M$, and $\Sigma_M\, p(\bullet) = 1$. Once the first-ranked individual is chosen, he or she determines the second alternative on the agenda, y_2: call this proposal $b(1)$. Once $b(1)$ is revealed, the second individual under π is selected by a second lottery from the remaining $m - 1$ eligible legislators, and this individual then determines y_3, denoted $b(2)$. In general, given the sub-sequence $\pi_{s-1} = (i_1, i_2, \ldots, i_{s-1})$, where i_r is the rth-ranked individual in π, and given proposals $\{b(0), \ldots, b(s-1)\}$, the sth-ranked legislator is chosen by lottery from legislators in $M\backslash\{i_1, \ldots, i_{s-1}\}$, $s = 2, \ldots, m$. For any $j \in M\backslash\{i_1, \ldots, i_{s-1}\}$, the probability that j is selected to make proposal $b(s)$ is given by $p(j = i_s \mid \pi_{s-1}) \in [0, 1]$: by assumption, $\Sigma_{M\{\cdot\}}\, p(\bullet|\bullet) = 1$ (so that $p(j = i_s \mid \pi_{s-1}) > 0$ for at least some $j \in M\backslash\{i_1, \ldots, i_{s-1}\}$). So while the lottery at any stage s can depend on the subsequence π_{s-1}, it is presumed independent of the proposals $\{b(0), \ldots, b(s - 1)\}$. By convention, write $p(j = i_1) \equiv p(j = i_1 \mid \pi_0)$, $\forall j \in M$. By definition, $\pi = (i_1, \ldots, i_m)$ and $\forall j \in M$, $\exists\, s \in \{1, \ldots, m\}$ such that $j = i_s$.

All the probabilities are common knowledge and can be used at each stage of the process to compute the likelihood of any given sequence, π, occurring. Let $\mu(\pi \mid \pi_s)$ be the probability that the de facto sequence of proposers is π, given the realized subsequence π_s, $s = 0, \ldots, m - 1$. For example, if $M = \{1, 2, 3, 4, 5\}$ and $\pi_2 = (4, 2)$, then

$$\begin{aligned}\mu[(4, 2, 1, 3, 5) \mid \pi_2] &= p[1 = i_3 \mid (4, 2)] \\ &\bullet p[3 = i_4 \mid (4, 2, 1)] \\ &\bullet p[5 = i_5 \mid (4, 2, 1, 3)] \\ &= p[1 = i_3 \mid (4, 2)] \\ &\bullet p[3 = i_4 \mid (4, 2, 1)].\end{aligned}$$

Evidently, $\mu(\pi \mid \pi_{m-1}) \in \{0, 1\}$ for all π.

At any stage $s \geqslant 1$ in the agenda-setting process, if the sth legislator (i_s) does not wish to offer any alternative at all *or* any not already proposed, then $b(s) = \emptyset$ (where, by an abuse of notation, $\emptyset \in X$): under the amendment procedure, this captures the parliamentary rule preventing reconsideration of previously eliminated alternatives. The final agenda, then, is at most of length $m + 1 \leqslant n + 1$ and is given by $\mathbf{b} = [b(0), b(1), \ldots, b(m)]$. Once $\mathbf{b}$ is determined, voting proceeds according to the amendment procedure to arrive at the committee decision.

To save on notation, given any subsequence π_s, $s \leqslant m$, relabel individuals in N so that $i_r = r$, $\forall r = 1, \ldots, s$. For every $s = 1, \ldots, m$, let $B_s = \{b(0), \ldots, b(s - 1)\}$. A *(pure) proposal strategy* for individual $i \in M$ is then a function: b_i: $X^i \rightarrow (X \backslash B_i) \cup \emptyset \equiv \beta_i$. For legislators $j \in N \backslash M$, define the (vacuous) proposal strategy, b_j, by setting $\beta_j \equiv \emptyset$.

Let $|\Sigma|$ be the number of possible pairwise contests identified by branches of the voting tree, Σ. A *(pure) voting strategy* for individual $i \in N$ is a function: v_i: $\Sigma \rightarrow \{0, 1\}^{|\Sigma|}$. In any pairwise contest, a value *1* denotes a vote for the alternative proposed later in the agenda (i.e., with the larger index under π), and a value of *0* denotes a vote for the alternative proposed earlier.

Since a legislator will cast at most m votes during the voting process, i's voting strategy can be decomposed, namely, $v_i = \{v_{it} \mid t = 1, \ldots, m\}$, where t indexes the voting stage ($t = m$ being the final vote). Legislator i votes *sincerely at stage* t, $t = 1, \ldots, m$, if and only if for any $j > m - t$,

$$u_i[b(j)] > (<)\, u_i[b(m - t)] \rightarrow v_{it}([b(j), b(m - t)]) = 1\ (0)\ ).$$

If ever a legislator is indifferent and sincere at stage t, assume he or she votes surely for the alternative with the higher index under π (with endogenous agenda setting, this turns out to be the unique equilibrium strategy [Banks and Gasmi 1986]). Legislator i is said to vote *sincerely* if and only if i votes sincerely at every stage $t = 1, \ldots, m$.

A *(pure) strategy* for legislator i is thus an ordered pair, $\sigma_i = (b_i, v_i)$, $i \in N$. Let $\sigma = (\sigma_i)_N$ and $\sigma_{-i} = (\sigma_1, \ldots, \sigma_{i-1}, \sigma_{i+1}, \ldots, \sigma_n)$. Given any list of strategies, σ, let $y(\sigma)$ be the final outcome of the decision-making process. The noncooperative solution concept used for the extensive-form committee game induced by the structure described above can now be defined. An *equilibrium* is a list of strategies $\sigma^* = (\sigma_i^*)_N$ such that, $\forall i \in N$:

1. $b(i)^* = b_i^*(\bullet)$ maximizes $Eu_i[y[(b, v_i^*), \sigma_{-i}^*]]$ over β_i, where the expectation operator E is with respect to the sequence π.
2. $v_i^* = \{v_{it}^*\}$ is such that $\forall t = 1, \ldots, m$, conditional on i being pivotal at each stage t, v_{it}^* maximizes $u_i[y[(b_i^*, v), \sigma_{-i}^*]]$ over $\{0, 1\}^{|\Sigma|}$.

Because there is complete information and decision making is by the amendment procedure, in equilibrium legislators will adopt sophisticated voting strategies in the sense of Farquharson (1969), and McKelvey and Niemi (1978). So, taking this as given, it is enough to consider the agenda-setting stage of the game.

For any policy $x \in X$, define the *win set of* x, $W(x) = \{y \in X \,\|\, \{i{:}u_i(x)|\} > n/2\}$, and let $W^c(x)$ be its closure. If $W(x) = \emptyset$ for any alternative x, x is a *Condorcet winner*. By the assumptions on individual preferences, for any alternative x, $W(x)$ is open. Therefore, if ever an individual is constrained to choose an alternative in $W(x)$ and $W(x)$ is nonempty, i's maximization problem is not well defined. However, by the observation made earlier regarding equilibrium voting strategies for pairs of alternatives over which an individual is indifferent, the statement, "Maximize over $W(x)$" can always be replaced (without loss of generality) by the statement, "Maximize over $W^c(x)$," which is well defined. Hereafter, this is left implicit.

THEOREM. *Suppose there is no Condorcet winner in X. Then there exists a unique equilibrium σ^* to the game, and the observed voting behavior of all legislators in equilibrium is sincere.*

Proof. To save on notation (and without loss of generality), set $M = N$. For any legislator j, define, $Q(j) = \{\cap W[b(s)]\} \cup \emptyset$, where the intersection is taken over all $b(s) \in B_j$. Given π_{n-1} and $B_n = \{b(0), \ldots, b(n-1)\}$, legislator i_n offers the final proposal. Without loss of generality, set $i_n = n$. Hence,

$$b_n^*(B_n) \in \{b(n) \in \beta_n \mid b(n) \in \mathrm{argmax}_{Q(n)} u_n(b)\}$$

is, given sophisticated voting, a best response for legislator n. Given π_{n-2}, the individual offering the penultimate proposal knows surely the identity of the final proposer, i_n ($= n$, by assumption). Moreover, n's proposal strategy is uniquely defined. Again without loss of generality, label N so that $i_{n-1} = n - 1$. Then legislator $n - 1$ can do no better than to adopt

$$b_{n-1}^*(B_{n-1}) \in \{b(n-1) \in \beta_{n-1} \mid b(n-1) \in \mathrm{argmax}_{Q(n-1)} u_{n-1}[b_n^*(\{b\} \cup B_{n-1})]\}.$$

Given π_{n-3}, there are two possible de facto sequences π that can occur, depending on the outcome of the final lottery over $\{n, n-1\}$ to determine who proposes the penultimate alternative on the agenda. But whichever sequence occurs, the proposal strategies of the remaining proposers are well defined. Therefore, individual i_{n-2} (identified by assumption with $n - 2$) has a best response to adopt,

$$b_{n-2}^*(B_{n-2}) \in \{b(n-2) \in \beta_{n-2} \mid b(n-2) \in \mathrm{argmax}_{Q(n-2)} Eu_{n-2}(\bullet)\}$$

where

$$Eu_{n-2}(\bullet) = \Sigma_\pi \mu(\pi \mid \pi_{n-3}) \bullet u_{n-2}[b^*_{m(\pi)}(b^*_{(m-1)(\pi)}(\{b\} \cup B_{n-2}))]$$

and $m(\pi)$ is the final proposer under π, and so on. Proceeding iteratively in the obvious manner, define analogous proposal strategies for all $i_{n-3}, \ldots, i_1$. By construction, this set of proposal strategies is well defined, constitutes an equilibrium, and is the unique such set. However, by sophisticated voting, for some $Q(j)$, j's best payoff can be invariant across several feasible proposals, or none. Therefore, there may exist several equilibrium agendas induced by σ^*.

In the equilibrium, voting strategies are sophisticated. Consider voting behavior along the equilibrium path for any agenda $\mathbf{b}^*$, derived from the specified proposal strategies. At the final nonvacuous voting stage of this path—that is, $b(s)$ versus $b(0)$, $n \geqslant s \geqslant 1$ and $b(s) \neq \emptyset$—all legislators surely vote sincerely. Because there is no Condorcet winner, $W[b(0)] \neq \emptyset$, and such a stage exists. By construction, $b(s) \in W[b(0)]$, so $b(s) = y(\sigma^*)$ is the final outcome. Consider the penultimate voting stage in which $b(1)$ is put against an alternative $b(j) \neq \emptyset$, $n \geqslant j \geqslant 2$. Then, $y(\sigma^*) \in \{b(j), b(1)\}$. If there is no such alternative, we are done. So suppose there is a nonvacuous alternative. If $y(\sigma^*) = b(1)$, then, by the proposal strategies, $W[b(1)] = \emptyset$ implying $b(1)$ is a Condorcet winner: contradiction. Hence, $y(\sigma^*) = b(j)$. By construction, $b(j) \in Q(j)$. Clearly, any individual $i \in N$ for whom $u_i[b(j)] > \max\{u_i[b(1)], u_i[b(0)]\}$ will vote sincerely at this stage. Similarly, since $b(1)$ and $b(j)$ are in $W[b(0)]$, any individual i for whom $\min\{u_i[b(1)], u_i[b(0)]\} > u_i[b(j)]$ has sincere voting as a weakly dominant strategy here. Suppose $i \in N$ is such that $u_i[b(1)] > u_i[b(j)] > u_i[b(0)]$. Then because $b(1) \in W[b(0)]$ by construction, i has a weakly dominant strategy to vote sincerely. Finally, suppose $i \in N$ is such that $u_i[b(0)] > u_i[b(j)] > u_i[b(1)]$. By construction, $b(j) \in W[b(1)] \cap W[b(0)]$ and $b(1) \in W[b(0)]$. So it is again a weakly dominant strategy for i to vote sincerely at this stage. Hence sincere voting over $\{b(j), b(1)\}$ is a weakly dominant strategy for all individuals. By sincere voting at the final stage, definition of $b(j)$ implies all legislators vote sincerely at this penultimate stage. Now apply an obvious inductive argument to complete the proof. QED

COROLLARY. *Suppose there exists a Con-*

dorcet winner, x^*, in X. Then there exists an equilibrium σ^* to the game, which is unique up to the proposal of x^*. The observed voting behavior of all legislators in equilibrium is sincere, except possibly at voting stages up to and including the first vote involving x^*.

Proof. For any legislator $j \in M$ such that $Q(j) \neq \emptyset$, the proposal strategy specified in the proof of the theorem is the best response. Suppose there exists $i \in M$ and $b(j) \in B_i$ such that $W[b(j)] = \emptyset$. Then $Q(i) = \emptyset$. Set $b(j) \equiv x^*$. By a theorem of McKelvey and Niemi (1978), x^* will be the final outcome under sophisticated voting whatever alternative i elects to propose. Hence, *any* proposal strategy is a best response for i. In particular, if $b(i) \neq \emptyset$ and $b(i) \notin \cap \{W[b(t)] \mid b(t) \in B_i$ and $W[b(t)] \neq \emptyset\}$, then sophisticated voting along the equilibrium path need not be sincere. Moreover, by the argument for the theorem, a necessary condition for voting not to be sincere at some stage for some legislator is that there be at least one such proposal $b(i)$. The corollary now follows from the theorem. QED

The corollary identifies the logically possible exception to the claim that, with endogenous agenda setting, sophisticated voting is always observationally equivalent to sincere voting. Having said this, three points should be emphasized. First, the conclusion of the theorem goes through if a Condorcet winner x^* is not proposed, even though it exists. Second, if x^* is placed on the agenda, then making no further proposals is equilibrium behavior, as is offering alternatives that can defeat (under sincere voting) all those previously proposed other than x^*. In both these cases, voting along the entire equilibrium path *will* be observationally equivalent to sincere voting. Thus, even when there exists a Condorcet winner x^*, there always exist equilibria to the game, in which sincere and sophisticated voting coincide along the equilibrium path; and these equilibria are unique up to the proposal of x^*. Third, in the spatial context at least, the existence of a Condorcet winner is generically confined to one-dimensional issues.

Finally, note that if there exists a Condorcet winner x^* in X and if all legislators are eligible to propose alternatives ($M = N$), then x^* is surely the outcome to the agenda game above. To see this, simply note that by Plott's (1967) theorem and n-odd, there exists an individual $j \in N$ with ideal point x^*. Therefore x^* will be placed on the agenda.

Conclusion

If committee decision making is by the amendment procedure and the agenda is determined endogenously (either by a subcommittee or by the committee of the whole), then the theorem offers an explanation for why legislators are observed to vote sincerely. Furthermore, along the equilibrium path, no individual will vote against his or her own proposal should this be put against an alternative offered earlier in the agenda-setting sequence.[3] This explanation does not rest on widening the domain of legislator preferences.[4] Nevertheless, it is worth reiterating that the observed behavior is supported by sophisticated voting *strategies.* In empirical work attempting to test for strategic voting, therefore, it is important to determine whether or not agenda setting is endogenous. If it is, then, ceteris paribus, the theorem predicts that a hypothesis of sincere voting will not be rejected.

The key to the result is that sophisticated voting over an agenda induces legislators to confine their proposals to alternatives that can beat—under *sincere* voting—the proposals offered earlier in the agenda-setting process. Since the formal structure of sophisticated voting is invariant across the class of binary agendas (McKelvey and Niemi 1978), this suggests that the theorem holds not only for the amendment procedure, but also for any binary agenda and (binary) agenda-setting mechanism.

Notes

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1. Formal definitions of the amendment procedure and related concepts are given in the next section.

2. For related models of endogenous agenda setting, see, for example, Banks and Gasmi 1986, McKelvey 1986, Miller 1985, and Miller, Grofman, and Feld 1986.

3. Unfortunately, the observation that legislators behave as if constrained to vote for their own proposals against those offered previously has not been formally documented. It is nevertheless a commonplace among students of Congress. In the case where a Condorcet winner, x^*, has been placed on the agenda, a legislator can ensure against having to vote for alternatives previously placed on the agenda against his or her own proposal—if this is made after the appearance of x^* on the agenda—only by proposing nothing or by offering an alternative that beats all previously volunteered proposals other than x^*. (Both constitute equilibrium strategies here.) Thus, if legislators, for whatever reasons, do feel obliged to vote for their own proposals against those offered previously, the logically possible exception (identified in the corollary to the theorem) to the claim that sophisticated and sincere voting observationally coincide with endogenous agenda setting vanishes.

4. It has been rightly observed that, just as the model here excludes "home-style" reasons for voting behavior, it excludes similar ("position-taking") reasons for proposal behavior. These may well prove important, giving rise to a distinct model of strategic agenda setting. Having said this, there is in fact some room for position taking in the model here, as the corollary makes clear. If there is an underlying Condorcet winner (x^*) and if this is put on the agenda before all eligible committee members have made a proposal, then subsequent proposers can "position take," since whatever they offer will lose to x^* under sophisticated voting.

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REVIEW ESSAYS

Women and Politics in Western Europe. Edited by Sylvia Bashevkin (Totowa, NJ: Frank Cass, 1986. 101p. $29.50).

Women of Europe: Women MEPs and Equality Policy. By Elizabeth Vallance and Elizabeth Davies (New York: Cambridge University Press, 1986. x, 180p. $37.50).

Women, State, and Party in Eastern Europe. Edited by Sharon L. Wolchik and Alfred G. Meyer (Durham, NC: Duke University Press, 1985. xiv, 453p. $42.50, cloth; $16.95, paper).

The study of women and the politics of individual European nations has been less developed than the study of women in U.S. politics for a variety of reasons. Truly comparative studies of women and politics have been even rarer. Yet more difficult to find are analyses of women in international politics and organizations. These three books, therefore, are bound to make a contribution to our knowledge despite some unevenness in their quality.

Women and Politics in Western Europe is a reprint of a special issue of *Westerrn European Politics.* It begins with an essay on the women's movement and the state by Jane Jenson that claims to be general in its focus but actually focuses on France. Of the remaining articles, one deals with the connection between the Italian women's movement and Communist party by Karen Beckwith; one is on women, the women's movement, and the French Socialist government (Wayne Northcutt and Jeffra Flaitz); and three are more comparative in nature, including Lawrence Mayer and Roland Smith's piece on electoral behavior and religiosity in Italy, West Germany, and the Netherlands, Ingunn Norderval's article on politics in Scandinavia, and Pippa Norris's study of legislative participation in Western Europe.

Women of Europe focuses on women and the European Community (EC) and European Parliament. Based on documentary and interview analysis, it considers both the role of women as participants in EC politics and European legislation as it affects women. The book provides necessary background material on the structure and processes of the EC and its relationship to member nations.

Women, State, and Party in Eastern Europe is an anthology based on papers presented to a 1981 conference on women in Eastern Europe that includes articles by both U.S. and Eastern European scholars. It is divided into five sections. Part 1, "Conditioning Factors," includes one article on Marxist and feminist theory (Meyer) and one on development and transformation (Wolchik). Part 2, on the pre-Communist period, includes two articles on Czech women (Karen Freeze and Bruce M. Garver), one on Ukrainian feminism in Poland (Martha Bohachevsky-Chomiak), and one on peasant women in Croatia (Mary E. Reed). Part 3, "Women and Politics," includes case studies of Romania (Mary Elen Fischer) and Albania (John Kolsti) and comparative studies of Romania and Poland (Daniel B. Nelson) and Czechoslovakia and Poland (Barbara Jancar). Part 4, "Women and Work," includes work on Hungary (Rozsa Kulcar, Ivan Volgyes), Yugoslavia (Silva Meznaric, Susan L. Woodward), and Czechoslovakia (Alena Heitlinger) and two comparative studies (Robert J. McIntyre and Bogdan Mieczkowski). Finally, part 5, "Women's Voices," looks at Poland (Renata Siemienska), Romania (Gail Kligman) and East Germany (Dorothy Rosenberg).

The research presented in *Women, State, and Party* employs a rich variety of sources and approaches, including theoretical treatments; historical and documentary evidence; census materials; surveys; and literature, song, and ritual explorations. The major sections of the book are introduced by synthesizing essays by Wolchik.

These three books, separately and together, raise a series of questions that are or should be central in the study of women and politics. All three include policy change as a major theme and provide interesting evidence regarding the relative roles of political organization mobilized on the basis of feminist politics and women's interests and that mobilized on the basis of other interests. It has long been noted that some of the most important changes in women's policy takes place in the absence of a strong feminist movement. This theme is taken up most directly in the two anthologies. Both raise the particularly interesting question of the uses "women's policy" serves for state and party. A related question, also a theme in the anthologies, is the organizational relationship between women's or feminist groups and party organizations. One wishes, however, that more attention had been paid in both volumes to distinguishing carefully between *women's* groups and *feminist* groups.

Both *Women of Europe* and *Women, State, and Party in Eastern Europe* focus our attention on one of the questions that is most unfortunately neglected elsewhere: the role of international politics in gender relations and policy. The agencies of the EC have gained importance in affecting the condition of women in member nations; we must be grateful to Vallance and Davies for taking up this topic. Many of the articles in *Women, State, and Party* underscore the need to recognize an international politics of gender in Eastern Europe, not just because of the relationship between the Soviet Union and Eastern European states, but because of the historical relationships among Eastern European communities.

The link between religion and gender politics is also a subtheme in both Bashevkin's anthology and Wolchik and Meyer's. Whether the gender relevance of politics and religion is viewed through the lens of the affinity between religion and some political parties of the West, as treated as Bashevkin's book, or through struggles between religion and state, as is often the case in Eastern Europe, it is a problem that needs more work. These two books serve as a useful corrective to feminist scholars whose understanding of gender, religion, and politics has been exclusively embedded in the peculiar relationship of religion to politics in the United States.

Each book, of course, has its own merits and problems. *Women and Politics in Western Europe* is uneven in quality. Its particular high points are Beckwith's rich article on the women's movement and party in Italy and the comparative syntheses of Scandinavia by Norderval and Western Europe by Norris. The most serious criticism is owed to the publisher: Frank Cass and Company should note that readers grow weary of paying 30 dollars for hundred-page books, especially when they are reprints of special issues of journals already published.

Women of Europe is a pathbreaking book because of its topic, but some of the research methods are disappointing. The book is based in part on interviews, but there is no discussion of how the respondents were selected or how the interviews were analyzed. One is often left with no sense that the choice was systematic or representative of anything in particular. There is considerable use of anecdotal reporting, which leaves the scholarly reader with no confidence in what observations to accept and what not to accept. There is a considerable literature on gender and politics available that should have been used but was barely touched. The study is largely descriptive rather than analytical or theoretical. Finally, Cambridge University Press must share the bouquet of onions for the price of the book.

Women, State, and Party in Eastern Europe is a very good book. The range of methods and topics makes one forgive a certain unevenness in quality. This book handles the tie between theory and ideology on the one hand and practice on the other in a way the other two books do not. It usefully joins historical and contemporary research. The existence between two covers of some articles based on rich and systematic Eastern European demographic and survey data (see especially the articles on production and reproduction) and one on oral poetry and ritual in Romania that is both provocative and lovely to read is impressive. It is also good to see people based in the United States and Eastern Europe cooperating in scholarship that deserves the name.

These three books, then, each deserve to be read on their own terms. But even more, they should be read together.

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Book Reviews: Review Essays

Why We Lost the ERA. By Jane J. Mansbridge (Chicago: University of Chicago Press, 1986. xii, 327p. $30.00, cloth; $9.95, paper).

Why ERA Failed: Politics, Women's Rights, and the Amending Process of the Constitution. By Mary Frances Berry (Bloomington: Indiana University Press, 1986. 147p. $12.95).

Rights of Passage: The Past and Future of the ERA. Edited by Joan Hoff-Wilson (Bloomington: Indiana University Press, 1986. xx, 140p. $22.50, cloth; $6.95, paper).

Serious attempts to amend the U.S. Constitution are rare. Even rarer are attempts to amend the Constitution that prove unsuccessful despite the mobilization of thousands of people and the investment of hundreds of thousands of dollars. On this basis alone, the proposed Equal Rights Amendment to the Constitution merits serious scholarly attention. However, the struggle over the ERA is important not only because of the insights it can offer about the amending process but also because of the central role it has played in one of the most significant social movements in U.S. history. Indeed, the ERA became the major focal point for the U.S. women's movement during the late 1970s and early 1980s.

Individually and collectively, these three books represent an important first step toward understanding the history and struggle surrounding the ERA. While all of the authors agree that in some ways the unsuccessful attempt to ratify the ERA was worth the effort, the three books take different approaches and offer distinctive explanations for the ultimate failure of the proposed amendment.

Jane Mansbridge argues that the ERA failed because a majority of citizens did not want major changes in gender roles. Both sides in the ERA dispute exaggerated the probable effects of the amendment, which Mansbridge concludes would have had little short-term impact on the status of women. As a result, many state legislators came to fear that the ERA might lead to major substantive changes, and they were reluctant to give the Supreme Court new language that might lead to more progressive decisions in the area of gender relations.

Mansbridge claims that the polarization of views evident in the ERA campaign and the exaggeration of the probable effects of the proposed amendment stemmed largely from the voluntary nature of movement participation. The need to mobilize volunteers and the reliance on their efforts fostered ideological purity, decisions by accretion, and lack of accountability, which ultimately worked against the proponents' goal of ratification. The positions taken by ERA proponents on the issues of women in combat and the relationship of abortion to the ERA illustrate these tendencies. Mansbridge suggests that feminists might have won more support from legislators if they had argued for a "deferential" interpretation on the question of whether the ERA would require women to be sent into combat. However, the argument that the war-powers clauses of the Constitution would lead the Supreme Court to defer to military judgment in military matters was never really seriously considered within the pro-ERA ranks. Rather, proponents automatically opted for an "egalitarian" interpretation, consistent with their commitment to the general principle that the law should be gender blind, and argued that women should be sent into combat on the same basis as men.

On the abortion issue, the relatively autonomous actions of feminist lawyers, who were not accountable to national leadership, were partially responsible for linking abortion to the ERA. In several states feminist attorneys brought suits arguing that restrictions on public funding of abortions were unconstitutional under due-process and equal-protection clauses of state constitutions and under state ERAs. The arguments put forward by feminist lawyers in state courts made it more difficult for feminists in unratified states to argue convincingly that a federal ERA would not require federal funding for abortion. Although Mansbridge claims that tendencies toward ideological purity, decisions by accretion, and lack of accountability can be countered by institutional arrangements that foster sustained internal dialogue within organizations, it is difficult to imagine a social movement that would not show these tendencies.

The major problem with Mansbridge's intriguing and provocative book is that it appears on the surface to be a more comprehensive and nationwide study of the ERA ratification effort than it really is. One has to search for a description of the study's methodology, which is found in a footnote to the preface. Although the author conducted almost a hundred interviews, the majority of these were with activists and legislators involved in the Illinois campaign. Of the interviews conducted with individuals outside Illinois, a significant number were with lawyers, and indeed one of the great strengths of this book is the insight it provides into the thinking and actions of attorneys involved in the ERA struggle. Despite Mansbridge's claim that the patterns and processes she observed in Illinois, the only nonsouthern state apart from Mormon Utah to fail to ratify the amendment, were "probably quite typical" (p. 177), one cannot help wondering if and how the book might have been different had her interviews focused primarily on the ERA struggle in Utah or Florida or Virginia. Despite an occasional quote from National Organization of Women President Eleanor Smeal, the perspective of national leaders in the ERA movement also seems largely absent from the book. Thus, *Why We Lost the ERA* is most appropriately read as an in-depth study of the Illinois ratification effort framed in the larger context of the national struggle over the ERA, and as such it makes an important contribution to our understanding of the politics of the ERA and of social movements generally.

Mary Frances Berry adopts an approach to examining the ERA quite different from that of Mansbridge. Berry's book places the ERA in historical perspective by comparing the circumstances surrounding the attempt to ratify the ERA with the circumstances surrounding past attempts, both successful and unsuccessful, to amend the Constitution. Specifically, she compares the ERA campaign with attempts to add to the Constitution amendments on the status of blacks, an income tax, prohibition, woman suffrage, and child labor.

Berry maintains that the defeat of the ERA was predictable when examined in historical perspective. The framers of the Constitution intentionally made it difficult to amend the document. Successful amendments have required a consensus that constitutional change is necessary not only at the national level but also within enough states to achieve ratification. Successful attempts to amend the Constitution have taken place during eras of reform, not reaction, and they have required a widespread perception that a social problem could not be remedied without constitutional change.

Proponents of the ERA were successful in building the national consensus necessary to obtain congressional passage of the amendment in 1972. However, proponents did too little too late in trying to build a state-by-state consensus. Moreover, in the decade following congressional passage of the ERA, the Supreme Court broadened its protection of women under the Fourteenth Amendment, making it harder for proponents to demonstrate the necessity of the amendment. Support for the ERA actually eroded during the final stages of the ratification campaign.

Berry seems more optimistic than Mansbridge about the future of the ERA. Unlike Mansbridge, she holds out the possibility that the ERA might someday be added to the Constitution. To this end, she concludes her book with one paragraph of general advice for ERA proponents based on her analysis of how and why past amendments succeeded, which leaves the reader longing for a more detailed and expansive examination of the implications of her argument for the future of the ERA. Nevertheless, Berry's scholarly caution does not seriously detract from the important and compelling argument of her book.

Like Berry's book, the goal of the volume edited by Joan Hoff-Wilson, clearly intended as a text for classroom use, is to place the defeat of ERA in historical perspective. An impressive array of historians, political scientists, and other scholars engaged in relevant research examine the origins of the ERA, the reasons for its defeat, and its legal and political impact. The section on origins includes informative essays on Alice Paul, who drafted the ERA and had it introduced into Congress in 1923, and Florence Kelley, a politically active social reformer who opposed the ERA. The defeat of the ERA is examined from many perspectives: as a cultural conflict over the meaning of womanhood, as a non-zero-sum game, as the result of proponents' reluctance to use civil disobedience and nonviolent direct action, and as the product of supporters' failure to devise effective persuasive appeals.

Unlike many edited volumes, the editor's introductions to this book and to its various sections not only link together the essays of the various contributors but also make an important contribution in their own right. Hoff-Wilson discusses the difference between feminism based on "radical individualism," as represented by ERA, and "relational" feminism, which seeks to preserve differences between women and men. She notes that many contemporary feminist scholars are now questioning whether equality based on individualistic and male notions of rights should be the major goal of contemporary feminism. Somehow, contemporary feminism must find a way to bridge the gap between these two distinct strains of feminism.

The books by Mansbridge and Berry are "must" reading for scholars interested in social movements, women, and politics, or the amending process. All three books are also potentially useful as classroom texts. Mansbridge's book would be particularly appropriate for courses on social movements, Berry's book should be considered as a supplementary text for courses on the legislative process, and the Hoff-Wilson reader would seem best suited for courses on twentieth-century U.S. political history. All three books deserve consideration for use in courses on women and politics.

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POLITICAL THEORY

Strong Democracy: Participatory Politics for a New Age. By Benjamin R. Barber (Berkeley: University of California Press, 1984. xvi, 320p. $9.95, paper).

In *Strong Democracy* Benjamin Barber argues powerfully for a government in which "all of the people govern themselves in at least some public matters at least some of the time" (p. xiv). With verve, style, passion, and insight, Barber explains how this ideal is possible, why we have never practiced it, and what conceptual and practical innovations might make it work.

Rejecting the "liberal" idea of a natural, prepolitical state whose inhabitants are endowed with liberty, equality, and rights, Barber insists, correctly, that we acquire these goods through the process of governing ourselves in common. Rejecting as well the static, aggregative liberal conception of citizenship, Barber insists that citizens become capable of common purpose through the process of common governance rather than through the simple coincidence of preexisting interests. Such a politics involves activity, energy, and work. It involves institutions that help people create "public ends where there were none before" (p. 152), and individual interests that will "change shape and direction when subjected to these participatory processes" (p. 152). It involves a concept of political knowledge that is provisional, evolutionary, and mutable, "produced by an ongoing process of democratic talk, deliberation, judgment, and action" (p. 170). It involves a political judgment that is neither "subjective" nor "objective," but rather proceeds from the "kind of 'we' thinking that compels individuals to reformulate . . . 'I want *x*' . . . as '*x* would be good for the community to which I belong'—an operation in social algebra for which not every '*x*' will be suitable" (p. 171).

"At the heart of strong democracy is talk," Barber writes (p. 173). And the section on talk is the most persuasive in the book. Strong democratic talk, he tells us, "entails listening no less than speaking; . . . is affective as well as cognitive; and . . . its intentionalism draws it out of the domain of pure reflection into the world of action" (p. 174). Barber contends, correctly I believe, that representative democracy (which he sometimes calls "thin" democracy) diminishes this kind of talk, and, in doing so, dramatically reduces the meaning and effect of citizenship.

Barber concludes with a collection of imaginative suggestions for adding strong democracy to the primary representative institutions of large-scale modern societies. He argues for "common work and common doing"—collectively creating pocket parks, urban farms, storefront community-education centers, neighborhood skill teams, crime-watch units, and universal-citizen service. He proposes neighborhood assemblies, for populations of from 5 to 25 thousand citizens, which could deliberate, vent grievances, act as ombudsmen, and possibly form part of an initiative and referendum process. He suggests representative town meetings, office holding by lot, and handling decriminalized small offenses through new forms of lay justice. He proposes a national initiative and referendum process, which would include a mandatory tie-in with neighborhood assemblies and interactive-television town meetings for the purpose of civic education, a multichoice referendum format (in which citizens would have a choice among *support strongly, support but with [a specified] objection, oppose strongly, oppose the proposal, but not the principle behind it,* and so forth), and a two-stage voting process providing for two readings. He supports experimental electronic balloting, vouchers in schooling and housing (with some reservations), universal-citizen service, neighborhood-action programs, workplace democracy, and a program for redesigning public space. If we tried all these things at once, as Barber recommends, citizens would undoubtedly "fly to the assemblies"—at least until the novelty wore off. Even after ennui set in, the intellectual and emotional residue would be incalculable. Why could not a state like Oregon or a town or an innovative university adopt some of his agenda on a trial basis, building in, of course, the prudence and the ways to "institutionalize regret" (p. 308) that Barber suggests?

Barber's repudiation of static liberalism and his creative suggestions for an ever self-renewing "strong" democracy are the best parts of this book. The worst side emerges when, in order to establish a conceptual foundation for strong democracy, Barber distorts opposing views in a way that is woven so tightly into the analysis that it undoes much of the good I have described. For all Barber's preaching about "empathy" and "listening" in his sections on democratic talk, he does not himself seem to have learned to listen. He hears what he wants to hear, and when the reality is otherwise, he distorts to fit. In this respect, *Strong Democracy* sometimes reads like a series of polemics against some of Barber's favorite bugaboos, including representative government and anarchism. In this, as in his past works, he argues by destroying straw monsters—caricatures of ideas that their adherents would never recognize.

Twelve years ago Barber gave to a photograph of uniformed officers subduing a citizen the simple caption, "Representative Democracy" (*The Death of Communal Liberty* [Princeton: Princeton University Press, 1974], 275). Nothing quite that blunt this time. Today he writes only that "the representative principle steals from individuals the ultimate responsibility for their values, beliefs, and actions" (p. 145), is "incompatible with freedom" (p. 146), "precludes the evolution of a participating public in which the idea of justice might take root" (p. 146), and produces "distrustful, passive" citizens (p. 219). That's it for representative democracy. Barber does not later qualify the words *steals, incompatible,* and *precludes.* He does not write an aside granting that every citizen wants and needs representation for some purposes at some times. He gives no hint, let alone discussion, of the kinds of public talk that representatives encourage and that their election inspires. This approach makes it hard to learn much about representative democracy from Barber's analysis.

Anarchism, another old enemy, receives similar treatment. Barber pulls one strand out of a complex (even inchoate and internally contradictory) set of ideas and concludes that "the anarchist perspective is the perspective of the radically isolated self for whom the world is only what it can see with its own eyes. . . . The anarchist reads the world as the realm of One [Self], where the existence of other Ones is scarcely perceived and never felt" (p. 37). And so on. Not a word on the communitarian anarchist tradition.

Although Barber deplores the present-day adversary tradition in political discourse, his own pen has been tempered in that forge. His style thus undercuts his own purpose. Rhetorical overstatement and denigrating the enemy may be required to infuse public debate with a new vision as important as this one. But the style is not compatible with the respect for others required by "strong democratic talk."

JANE MANSBRIDGE

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Book Reviews: Political Theory

Reciprocity. By Lawrence C. Becker (New York: Routledge & Kegan, 1986. 436p. $32.50).

Although the death of political philosophy was widely predicted a generation ago, the renewed vigor of the field is epitomized by Lawrence Becker's *Reciprocity*. While many political theorists may disagree with the mode of approach, few would deny that it reflects the spreading interest in philosophical analysis to determine the foundations of political virtue.

Becker writes in the neo-Kantian tradition of "*a priori* moral theory," based on what he describes as "the general conception of morality." In this view, "moral judgments are judgments about what rational agents ought to do or be, period" (p. 17)—and such judgments are determined by discovering "determinable rules of participation, teleology, deontology, valuation, procedure, transformation and recognition" that satisfy the "conditions for logical validity and soundness" determined by contemporary philosophers (p. 36).

While the abstract quality of this fundamental approach may strike many political scientists as difficult, Becker seeks to provide concreteness through an unusual series of devices. Part 3 is a scholium, with lengthy bibliographic essays on a wide range of scholarly fields. Interspersed in his text are many novelistic epigraphs "meant to bring vividly to mind the intricate, subtle, and particular lives within which, and for which, moral philosophy is done"; while "nothing in the argument formally depends on them," these epigraphs are also a means of providing concreteness (p. xi).

Becker's main thesis is that the abstract logical criteria for a "general theory of morality" lead to a "virtue theory" based on reciprocity. His definition of reciprocity, however, differs from that in most social psychology and game theory (pp. 354–68). For Becker, reciprocity must be the "disposition" to reciprocate for benefits received, whether they have been requested or not; such reciprocation need not be narrowly "in kind," but it does rest on an asymmetry between good and evil. According to Becker, while "good received should be returned with good," maxims concerning evil are quite different: "Evil received should not be returned with evil" and "Evil received should be resisted" (p. 74).

In part 2, Becker spells out the implications of the resulting view of morality for families, for future generations, and for the law. Throughout, he addresses philosophic literature from the ancients to such contemporaries as Nozick and Rawls. In short, Becker's *Reciprocity* is a serious attempt to confront contemporary ethical issues from the neo-Kantian perspective, focusing on the need to return to the kind of theory of dispositions or virtues traditionally associated with Aristotle and more recently emphasized by Alasdair MacIntyre.

For me, such an approach encounters the fundamental difficulty inherent in most theories supposedly based on a priori logic. According to Becker, one of the first requisites of "well-defined" rules of any activity is that the participants need to be specified. Since "a rule is a moral rule if it describes what rational agents ought to do or be, all things considered," "participants are therefore also specified: moral action guidance is addressed *to* rational agents, *from* rational agents" (p. 30).

Since this "participation rule does not limit the class of rational agents to human beings, nor does it necessarily include them all," Becker himself admits that "the boundaries of the concept of agency are somewhat vague (with respect to the status of young children, for example, and some animals)" (p. 31). Further reflection leads to the realization that this admission calls into question Becker's entire theoretical apparatus.

On the one hand, it is manifest—both from the philosophical tradition and from observation—that many animals satisfy the criteria of purposive action and self-consciousness that are used by Becker to define "rational agency." To cite the most obvious example, Jane Goodall and Frans de Waal have given us incontrovertible evidence of such putatively "human" traits in chimpanzees.

Unless the use of verbal speech is formally required, an a priori theory of rational agency cannot be limited to humans. If so, the web of reciprocity should, on Becker's own grounds, extend to other animals—with consequences that he does not seem to draw concerning the relationship of humans to individual animals and the ecological systems of which they are a part. If not, the theory cannot claim to be a priori, since it is based on the concrete characteristics of *human* behavior, combining as it does feelings and emotions as well as verbal speech and social behaviors derived from millions of years of hominid evolution.

At the other extreme, of course, one could argue that "rational agency" only applies to those who reason "in the silence of the passions." Here, however, Becker must confront the opposite difficulty, perhaps best stated by Rousseau. In the *Second Discourse*, after describing the golden rule as "cette maxime sublime de justice raisonnée," Rousseau adds, "Quoi qu'il puisse, appartenir à Socrate, et aux Esprits de sa trempe, d'acquerir de la vertu par raison, il y a longtemps que le Genre-humain ne seroit plus, si sa conservation n'eût dépendu que des raisonnemens de ceux qui le composent" (*Ouvres Completes*, vol. 3, Bibliothèque de la Pleiade no. 169 [Paris: Éditions Gallimard, 1964], pp. 156–57).

In short, either Becker's theory applies to many animals as well as to human infants or it is only applicable to "Socrates and minds of his calibre." Such an approach therefore proves either too much or too little, leading to the suspicion that purely a priori approaches to "virtue theory" are inferior to the observation and reflection characteristic of Aristotle. No longer does it seem reasonable to ignore the role of emotion and feeling in the phenomena known as morality; no longer can an account of human obligations to the family, to future generations, or to the law be based on abstract logic. It is time to admit that, like behaviorism, philosophic a priori-ism has been rendered obsolete by contemporary research in the life sciences. As Robert Axelrod has already shown, evolutionary biology is a better guide to human reciprocity than attempts to deduce obligations from the kind of "reasoned theorizing" (p. 31) used by many contemporary philosophers.

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Research in Political Sociology, Vol. 2. Edited by Richard G. and Margaret M. Braungart (Greenwich, CT: JAI, 1986. xviii, 332p. $52.50).

This book is the second volume in an annual series of original contributions revolving around a dominant theme. The previous year's issue dealt with the development of nation-states, whereas this 1986 issue considers a germane topic: effectiveness and legitimacy. Twelve articles of relatively equal length are grouped together under four headings: *political structure and change, cultural politics, youth politics,* and *social and political conflict.* Within these generous boundaries, each contribution remains autonomous and distinct in terms of the approach and topic chosen.

The series opens with what I would call a theoretical hors d'oeuvre. I cannot say whether it simply stands as an abstract endeavor meant to ennoble (so to speak) what is to follow or whether it is the outline of more elaborate future developments. In any case, Jonathan H. Turner and Charles H. Powers submit no less than twelve propositions under four captions. Borrowing from such classics as Marx, Durkheim, and Weber together with episodic imports from Simmel, Spencer, and Pareto, the authors manage to outline converging or complementary principles of political organization. Right from the start, Turner and Powers are aware that such an attempt may elicit more than reticent comments. Marx, Durkheim, and Weber do not always mix well. But despite incompatibilities deemed by them only apparent, the authors rely on common denominators in the name of a to-be-developed "natural science of society." The principles are not necessarily meant to be original and some of them verge on the obvious. The intention is mainly to bridge a gap between the classics and contemporaries.

The articles coming after this theoretical opening belong to different genres of writing; some may be considered full-fledged research articles, others tend towards the essay type, and some stand in between.

Robert J. Antonio expounds on the limits of Max Weber's model of authoritarian bureaucracy, which he considers too monolithic and fatalistic. In contrast, Antonio tries to demonstrate from the ancient Roman Empire—which had been Weber's point of reference—that instead of being destined for a state of permanence, any such regime is likely, by its own action, to generate countervailing pressures from the ruled and to have to submit to changes in the long run. The phenomenon of clientelism is put in evidence, underlining a feature common to both ancient and modern modes of authoritarian bureaucracy. The whole argument counts on the existence of an inherent dialectic in the relationship between rulers and ruled.

In the line of systematic research, Ronald Inglehart pursues his postmaterialist saga and furnishes up-to-date confirmations of a trend that should, I imagine, one day lead to this postmaterialist minority becoming the majority devoted to the promotion of the values of self-expression, a better environment, and social solidarity. Inglehart's mode of argumentation is sufficiently flexible to take into consideration the ups and downs of the postmaterialist trend, and therefore more likely to convince skeptical readers. The argument, as we know, revolves around the hypothesis that people reach levels of material satisfaction that drive them to strive for more sublime values thereafter. Just as Marx bound society to the imperative of material needs, this approach resorts to a psychological reading of human social development. In direct affiliation with Maslow, Inglehart expounds a psychological historicism that sets goals close to the ultimate values of the Western world. Scarcity and abundance are more or less given an absolute character.

From data collected in France in the late 1970s, David Swartz takes a stand against the "managerial-revolution" school and opts for a "class-organization" perspective. The study suggests that different classes in France control different types of organizations but that organizational constraints also require members of the managerial elite to conform to specific educational credentials. Social closure is therefore exercised through the recognition of a double standard of selection: economic and scholastic capital (using Bourdieu's terminology). In order to modify the frequent impression of an overwhelming technocratic elite topping both private and public spheres, Swartz establishes a dividing line between a public technocracy—which comes from professional family backgrounds and the highly selective channel of the *grande écoles*—and a still impressive part of the private sphere reserved for an elite based on family wealth.

On cultural matters Donald Granbert and Sören Holmberg, using a social-psychology approach, gauge the degree of subjective ideological congruence of individuals with the party they prefer. Comparing Swedish with U.S. citizens, the study reports a stronger ideological correspondence between the Swedes and the political party of their choice than is the case for their U.S. counterparts. Such a disparity in ideological attachment would be largely due to the way the political competition takes place—the presidential contest, for instance, lending itself to personalized rather than ideological campaigns.

As to youth politics, Joyce M. Mushaben compares the "no future" generation of the 1980s in (West) Germany with the youth movements of that same country between the two world wars. The paper focuses on their respective contributions to German political culture while denying that the present condition is necessarily conducive to a Nazi form of regime. In opposition to Inglehart, Mushaben emphasizes the feeling of insecurity deriving from economic uncertainty, unemployment, environmental deterioration, nuclear policies, and the like. Claiming that today's German youth are excluded from the benefits of postmaterialism, the author wonders if the present policies of integration into society are adequate. Indeed, the essay is normative, at least in its conclusions. In contrast, there is the study by Vladimir Shlapentokh of attitudes and behavior of Soviet youth in the 1970s and 1980s. Seemingly well documented (I am not a specialist in the field), the paper comes close to the type of good survey a reader might read in The *Economist*, including, of course, the same underlying message.

The book concludes with monographs on social and political conflicts. After a disproportionately long historical background to the "process of class struggle" in Hawaii, J. A. Geschwender and R. F. Levine sum up in a few pages how, after World War II, the Democratic party was prevented by capital's "red-baiting" campaign from becoming a prolabour party. Next are Carol L. Schmid's considerations on Quebec. Having posited that to exist as a submerged nation, a group has to conform to a number of conditions regarding its identity and political will, the author arrives at the foregone conclusion that Quebec only partially fulfills these requisites. The book closes on E. Walsh and S. Cable's case study of movements engendered by the Three Mile Island accident. The authors discuss the complexity involved in any analysis of social-movement litigation.

The idea of an annual review of this kind on research in political sociology is excellent, even though the treatment of themes still opens the way to a dispersion of topics.

ANDRÉ J. BELANGER

University of Montreal

Politics, Self, and Society: A Theme and Variations. By Heinz Eulau (Cambridge: Harvard University Press, 1986. x, 567p. $39.95).

This is a splendid book. It brings together 20 of Heinz Eulau's principal papers—the earliest from 1958, 10 from the sixties, 8 more from the seventies, and 1 published here for the first time. As such, it is a most valuable reference. Harvard University Press has done the profession a service by assembling these scattered papers.

The theme of the subtitle is that "self and society are inextricably intertwined in political life" (p. 2). Working with an understanding that "it is the function of science to understand and interpret the world, not to change it" (p. 22), Eulau's analysis of his theme has been guided by explicit epistemological considerations. His principal concern has been with the problem of simultaneity. This results from a larger unit and its smaller subunits being simultaneously involved in any political action. Neither the larger unit nor the smaller units are ever free of each other. The larger unit—whether a group, organization, or nation—does not have any independent status apart from the conduct of individuals who comprise it. But in the real world of politics, it is the collectivity and not its individual members that is the effective decision maker.

In order to collect data and analyze the simultaneously acting units, Eulau distinguishes between the subject unit—by which he means the observational unit, the unit whose behavior is observed—and the object unit—the *explicandum*, the unit whose behavior is to be explained. This distinction is crucial because proper analysis requires bringing all of the object unit's properties to the same level. This may involve either reduction (in which case properties on a more inclusive level are treated as contextual) or construction (with properties on a less inclusive level being treated as distributive—or relational or structural—properties of the object unit).

"Unless the properties of collective actors are obtained through reduction and construction," Eulau rightly warns, "analysis of large units will inevitably remain literary, discursive, and speculative" (p. 95). But if there are perils in not adopting this research strategy, there are substantial rewards for doing so. "Macrolevel construction can go a long way in making microanalytic, psychology-based models of decision-making behavior relevant to political institutions and processes" (p. 450). Just as self and society are inextricably intertwined, so is analysis of behavior and institutions. Political behavior is not a separate field of investigation but an approach to the full gamut of political phenomena.

An ability to move easily across levels of analysis—to transform data gathered on one level for proper use on another—also confers an ease in movement along the micro-macro continuum. As one moves toward the micro end, empirical data are added that make for greater validity and reliability. As one moves toward the macro end, more significant questions can be addressed. But it is not necessary, Eulau says, to give up validity and reliability in order to achieve significance. An understanding of construction and reduction of data allows an analyst to have validity *and* significance on whatever level. Many arguments in political science result from a failure to grasp this crucial point.

Both of Eulau's most extensive research projects have been informed by a levels-of-analysis perspective. In the four-state legislative study (chap. 6) the data were gathered on the individual level and much of the analysis was also on that level. But the legislature was conceptualized on the institutional level as a determinate role structure that "linked the legislature as a subsystem with other [political] subsystems" and "institutionalized and resolved the social, economic, and political conflicts" (p. 203) with which the legislature had to deal. The levels-of-analysis perspective was even more explicit in the Bay Area study of 82 city councils (chaps. 10, 11, 14, and 15). Here again, data were gathered on the individual level, but the analysis was all on the group level, dealing with integral properties (such as the size of council), relational properties (such as cosponsorship, oppositional activity, affect, respect, and expertise), structural properties (such as bipolar, unipolar, and nonpolar councils) and contextual properties (such as city size).

In *Politics, Self, and Society* Eulau also expounds configurative analysis and developmental analysis. A configuration is an emergent whole. "Emergent wholes such as 'personality,' 'society,' or 'culture' are units that are something more than the sum of the items, elements, or traits that constitute them individually" (p. 249). The task of the analyst, then, is less one of tracing cause and effect among the elements than of discovering the particular configuration that emerges from the combination of elements. Thus Eulau is interested, for example, in the response style that results from setting a city council in a particular political matrix. And in his most recent analylsis, Eulau traces life space and social networks, discovering differences between day dwellers and night dwellers in their use of neighborhoods.

Developmental analysis is based on Lasswell's distinction between goal thinking (analysis and selection of values toward which decisions are directed), trend thinking (analysis of past tendencies and future probabilities), and scientific thinking (analysis of limiting conditions). Therefore, says Eulau, "the methodological problem is nothing less than to connect statements of value or preference, statements of fact, and statements of expectation" (p. 228). *Politics, Self, and Society* includes three developmental analyses: a discussion of policy maps summarizing council members' perceptions of their city's stage of policy development; a quasi-longitudinal, quasi-experimental design for comparative policy analysis; and one of the very few reanalyses of the Miller-Stokes data.

"The behavioral persuasion in politics," Eulau wrote in the early sixties, "has more than one approach, and there are many voices that speak in its name. But they all have in common a commitment to the study of man as the root of things political or, to put it more technically, to the individual person as the empirical unit of analysis" (p. 22). *Politics, Self, and Society* contains the fruit of his personal efforts. There is a little irony to its publication in the late eighties. We are now hearing about a new institutionalism, but if political scientists had paid closer attention to what Eulau was saying, we would already have a richer institutional analysis. For if Heinz Eulau's subject has been man, his object has always been politics.

JOHN H. KESSEL

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Rational Association. By Fred M. Frohock (Syracuse, NY: Syracuse University Press, 1987. xi, 201p. $27.50).

New dimensions of the debates over liberalism and its fate are difficult both to isolate and to advance interestingly. This slim volume does both. Frohock introduces the consistency problems of liberalism by assessing them through the lens of collective-choice theory, which simplifies many of the dilemmas confronting formal understanding of democratic "rationality" as treated in the literature. By attending to the nature of rationality throughout the book, Frohock is able to clarify the problems and introduce possible solutions that could revitalize what many think of as the dying theory of liberalism. His primary question deals with the puzzles surrounding the inconsistency "between individual rationality and collective outcomes" (p. ix). His goal is to avoid the discontinuity between *one* and *all* that plagues rational-choice theory without making the major countererror of adopting a holistic solution.

The problem, embedded in treating utility-based rationality as the ground for quantitatively reaching collective action, is the move from the individual to community (set forth in chap. 1). The analysis then proceeds, through assessment of individuals and structures as set forth in the prisoner's dilemma, to establish the importance of isolation of individuals as a background assumption of the puzzles (chap. 2), especially in a concluding contrast with Aristotle's corporate model—giving a hint about where we eventually will end. Frohock continues his arguments about the tensions arising from Arrow's theorem and exchange theory to conclude (chap. 3) that "there is no mediating device to transform the disjointed features of liberalism into the type of moral community liberalism seeks" (p. 67). His assessments of individuals and groups lead him to conclude his criticisms of liberal theory's linkages with the observation that different liberal theorists (Hobbes, Locke, and Rousseau) had different approaches to the dilemmas (clarified by his analysis of formal theorists), with Rousseau's general-will solution—adopting community will and morality as one's own—providing the "exit routes" for these dilemmas (chap. 4, p. 98).

Frohock's own skillful and easily followed route through formal theory is continued in his assessments of rationality and reasoning (chap. 5), and rational forms (chap. 6). The path, clear enough for undergraduate use even though it advances the literature, provides a framework for pursuing careful assessment of the options for treating the fit between collective-choice theory and moral decisions, for moving from the rational individual to appropriate grounds for collective commitments on specific actions. Perhaps the single most powerful dimension of the book is that it makes clear to both proponents and opponents of rational-choice analysis just what is missing in the effort to deal with liberal and democratic theories from the choice perspective. Traditional theorists troubled by rational analysis will better grasp what is missing in the formal approaches; formal theorists will see what conditions are not resolved within their theories. Defining *reasoning* as deliberation (dealing within the realm of uncertainties)—as isolation of things that count as good reasons—rather than counting reasoners under a democratic motif permits one to move the discussion out of the constraints of formal choice of individuals (which leads to inconsistencies in collectivizing the will) into individual choices within the discourse that provides reasons for collective action that can be shared: "The task of political inquiry, once individuals are accepted explicitly as reasoning agents, is to recognize the juridical forms that are compatible with individual minds. . . . Individuals who are rational on reasons must rely on rules that govern; and the unity of minds can be located in the rules by means of which self-legislation occurs" (p. 159).

In his final chapter (chap. 7), Frohock carefully focuses on the forms of background assumptions that affect collective-choice applications to liberal values: "These three types of connections between individuals and collectives—decisiveness, fairness, and hypothetical generalization argument—introduce collective considerations to individual choice" (p. 169). The fairness option, however, is different in that it must provide a reason for cooperating. Fairness as a necessary but not a sufficient condition, for a moral association relies upon the giving of reasons: "A reason-giving form of rationality is the substance on which such an association is formed. Reasons, as we know by now, (a) are formed on a network of rules that can organize communities authoritatively, (b) define individuals holistically as members of rational classes, and (c) provide evaluative criteria to address those issues of fairness in the absence of which cooperation has no meaning" (p. 171).

Frohock provides convincing analysis that confirms the notion that reasons for choice within a collective are the key to rational association. Reasons, not just preferences, permit the establishment of rules and morality within the collective (p. 179). Deliberation, giving reasons and counterreasons while the collective works towards standards that can be shared, is an operation of reason that permits one to join a search for common interest in ways impossible under formal choice theory based on isolated preferences. In his concluding advocacy of "juridical democracy" (p. 181), Frohock has carried us back through *reasons for* returning to Aristotle's analysis from within formal theory's own logic—an important journey.

GEORGE J. GRAHAM, JR.

Vanderbilt University

Hermeneutics and the Sociology of Knowledge. By Susan J. Hekman (Notre Dame, IN: University of Notre Dame, 1986. 224p. $29.95).

Susan J. Hekman's *Hermeneutics and the Sociology of Knowledge* has two main goals: first, to point out the similarities between Hans-Georg Gadamer's hermeneutics and Karl Mannheim's sociology of knowledge and second, to indicate the implications of these similarities for social-scientific method. In her view, Mannheim and Gadamer share three notable insights. First, both emphasize interpretation. While Mannheim is confused on this point he nevertheless advocates a "hermeneutic method" according to which historical phenomena are to be investigated "in terms of the understandings and presuppositions of the age in which they are manifest" (pp. 69-70). For its part, Gadamer's *Truth and Method* attempts to show that interpretive understanding is fundamental to all aspects of human life. Second, both emphasize the way in which interpretation is conditioned by the situation of the interpreter and thus suggest the way in which Enlightenment conceptions of truth and objectivity need to be modified. Hekman contends that Mannheim is, again, somewhat confused on this point, calling his views phenomenological positivism. Still, like Gadamer, he shows that there is no ahistorical, unsituated Archimedean point from which to acquire an objective explanation or the truth of human beings or society. Rather, the worth of any interpretation is relative to the circumstances from which it is advanced and to the questions it tries to answer. For this reason, both Mannheim and Gadamer stress the importance of what Hekman calls *self-reflexivity*: interpreters must be aware that their perspectives are "prejudiced" (in Gadamer's words) and hence must be willing to subject them to critical examination. Finally, both Mannheim and Gadamer conceive of interpretive perspectives in "collective" rather than individual terms. In a passage that Gadamer might have written, Mannheim therefore asserts, "Strictly speaking, it is incorrect to say that the single individual thinks. Rather it is more correct to insist that he participates in thinking further what other men have thought before him" (p. 80).

What Hekman sees as the chief difference between Mannheim and Gadamer might be expressed in similar terms: namely, that Gadamer simply extends Mannheim's thoughts in a more radical and consistent manner. Not only is he clearer on the role of interpretation and its implications for Enlightenment views of truth and objectivity, he also clearly "reverses Enlightenment thought on the relationship between the natural and the social sciences" (p. 107). Neither theorist thinks that the methods of the natural sciences can provide any model for those of the social sciences. But Gadamer goes further, suggesting that insofar as the natural sciences disregard the conditions of prejudice, they are themselves flawed. As Hekman continues, "the model of understanding characteristic of the human sciences is the universal process by which we, as human beings, attain knowledge; that characteristic of the natural sciences, on the other hand, seriously distorts this process, hence calling into question the legitimacy of this mode of understanding" (p. 107). From this analysis, Hekman concludes that Gadamer's hermeneutics provides an important "anti-foundational" basis for the social sciences. Rather than universalizing notions of truth and objectivity developed for the natural sciences and seriously distorted in themselves, a hermeneutic social science focuses on the relations between "human thought and human existence" (p. 11).

As far as I know, Gadamer never charges natural-scientific method with internal flaws. His claim is rather that the mode of experience proper to the natural sciences is *not* proper to other fields of human inquiry and that their project is one rooted in the concerns and commitments of a particular community. Hekman's failure to clarify the point she is making here is indicative of similar failures throughout her book. She covers a great deal of ground, makes numerous distinctions and comparisons and contends with much commentary on, and criticism of, the texts with which she deals. But she does much of this far too quickly and therefore leaves significant gaps in her argument. In the remainder of this review I shall briefly consider two.

First, despite her intention, Hekman fails to spell out the implications of Gadamer's work for social-scientific method. She thinks that Gadamer goes beyond the Wittgensteinians in overcoming Enlightenment-type distinctions between natural and social science; for, although the latter emphasize interpretation, they continue to identify the natural sciences with the search for truth and simply exclude the social sciences from it. Gadamer, however, denies that either attains truth. I think there are misconceptions in this account. Of concern at this point, however, is the relatively superficial character of Hekman's exploration of the issues. If both natural and social science employ interpretations, if both are historically situated, is this all that needs to be said? What of that which Anthony Giddens has called the "double hermeneutic" in the social sciences? If Gadamer thinks that neither natural science nor social science attains "truth," what does he mean by talking about truth and denying the exclusive access of method to it?

This last question is related to another difficulty in Hekman's argument. Throughout her book she denies that Gadamer has a problem with truth because he defines it in terms of the collective conceptions or shared vocabulary of a community. Indeed, she claims that this is what separates Gadamer from the nihilism of Rorty, Foucault, and Derrida: we can make moral judgments and can distinguish between good and bad interpretations in general because we belong to historical traditions to which we can appeal for our normative standards. But what if our traditions are Fascist, racist, sexist or the like? It is on precisely this point that much of the criticism of Gadamer's work has focused. The question is how we examine our shared understandings or reflect on our prejudices. Hekman touches on this question at two points: first, where she discusses the self-reflexivity she finds in both Gadamer and Mannheim and second, in her discussion of Habermas's critique of Gadamer. In the first instance, however, she merely notes that both theorists demand self-reflection without investigating the conditions of its possibility. The problem here is that if all interpretation is prejudiced then so are the interpretations in which we reflect upon the prejudiced character of our interpretations. Hekman herself makes this point in discussing Gadamer's antifoundationalism. Still, if he is antifoundationalist in this regard, what are the consequences for his demand for self-reflection? If our prejudices are racist, will the vocabulary in which we reflect on them not also be racist? Habermas argues that "objectifying" methods need to be brought into the social sciences at precisely this point to allow distance from our own preconceptions. Hekman dismisses this solution, but in doing so she apparently overlooks the difficulty with her own assessment of Gadamer's work.

The attempt to synthesize Mannheim and Gadamer to provide the social sciences with "both a solid philosophical foundation and a viable research program" (p. 11) is interesting and productive. One wishes that Hekman had had time to explore the issues involved in more depth.

GEORGIA WORNKE

Radcliffe College

The Political Ideas of Marx and Engels, Vol. 2, **Classical Marxism, 1850–1985.** By Richard N. Hunt (Pittsburgh: Pittsburgh University Press, 1984. xiii, 421p. $37.95).

This is the successor volume to Hunt's well-received study of the political ideas of the young Marx and Engels, and a fitting memorial, given his untimely death, to its author. In his first volume Hunt assumed that the notion of the "parasite state" hovering above civil society constituted the core of Marx and Engels's early political thought. Here he traces in particular the development of a notion of "democracy without professionals," in which large-scale participation and the frequent election of all important officials were to prevent the growth of any permanent, alien political and bureaucratic force. This, Hunt presumes, was the chief achievement of Marx and Engels's later political thinking, the source for their adoption of the model of the Paris Commune, and the main distinction between their work and more authoritarian forms of twentieth century Marxism. Like his chief competitor, Hal Draper (*Karl Marx's Theory of Revolution* [New York: Monthly Review Press, 1977]), Hunt denies that anything like "totalitarian democracy" (in Jacob Talmon's phrase) was the natural outcome of Marx and Engels's critique of the liberal state. Analytically, Marx and Engels's notions of both the parasitic, or independent, state and the class, or instrumental, state developed in the 1850s through their observations of Bonapartism and studies of "Oriental despotism," the latter being gradually seen as the first instance of the parasitic state. After detailing these anthropological explorations, Hunt treats the development of both state theories up to the 1870s and the Paris Commune, to which two chapters are devoted. After a useful excursus (which will fuel the debate on Marxism and morality) into the problem of the degree to which Marx and Engels recognized individual rights and the question of the tyranny of the majority, Hunt examines the notion of classless society and the disappearance of the state, arguing that both have been misconstrued as completely utopian. His final chapters then detail Marx's debate with the anarchists in the International and the debates after 1870 over the possibility of skipping historical stages to reach socialism (notably relevant for Russia) and over the strategy of a legal, peaceful road to socialism in democratic countries. Hunt's is a careful, largely textual and very scholarly study. Abjuring ideology and polemic, he nonetheless rejects many well-known interpretations of areas of the topic (e.g., Wittfogel on Oriental despotism). His aim is not to expose Marx and Engels's weaknesses or heap the sins of their disciples upon them, but inevitably some questions of this type must be faced. Hunt indicates, for example, Marx's failure to foresee the expanded role of managers and bureaucrats in the future (cf. pp. 252–63), but neglects to ascertain why this (obviously serious) weakness in the theory persisted (more attention to Saint-Simon and to the problem of economic administration would have been helpful). Similarly he steers clear of some of the more unpleasant innuendoes associated with "proletarian dictatorship" (better handled by Draper). Nonetheless, this is an excellent study with great strengths, and its subtlety and toleration of the ambiguity of the sources help to raise the level of scholarship in this area considerably.

GREGORY CLAEYS

Washington University

James Madison: Creating the American Constitution. By Neal Riemer (Washington: Congressional Quarterly, 1986. xix, 203p. $11.95, paper).

This work, though not labeled as such, is apparently a revised edition of the author's earlier (1968) treatment of James Madison's political thought. Its seven chapters focus on concerns that stem from the question Riemer poses in the first sentence of the book: "Is republican government in a large state possible?" (p. 1). These chapters explore four difficulties—"disunion, large size, faction," and the "anti-republican danger"—that Madison confronted in his efforts to "reconcile liberty and large size in the infant American republic" (p. 132). As such, the subtitle is a bit misleading because these concerns take us well beyond Madison's efforts toward a stronger national government or his contributions to the Philadelphia Convention and the ratification struggle. Indeed, as Riemer makes clear at the outset, he is concerned with Madison's "evolving thought" throughout "his entire life," particularly his "maturing political conceptions," such as his "bold assertion of civil liberties, the organization of a republican political party, and a strategy of constitutional interpretation . . . that would safeguard union and freedom against anti-republican attack" (p. 3).

A goodly portion of Riemer's treatment is devoted to accounting for Madison's changes of heart regarding the nature of the union and the relative powers of the national government vis-à-vis the states. According to Riemer, Madison's strong nationalism, evident at the convention and in his early political thinking and activities, was modified as he came to realize that ratification of the Constitution required "a more moderate nationalistic interpretation" (p. 89). In Riemer's view, his modification on this and other matters, such as political parties, clearly manifest Madison's prudence—"a practical wisdom," "a quality of mind" concerned with "wisely relating means to ends" (p. 13). Thus, as Riemer would have it, many of Madison's presumed inconsistencies can be attributed to his "consistent prudential approach to politics" which rendered it difficult, if not impossible, "to maintain a superficial consistency" (p. 16). And where, in retrospect, many find Madison to be imprudent, to have taken extreme positions—for instance, his constitutional opposition to the national bank or the underlying theory of his Virginia Resolutions, drafted in response to the Alien and Sedition Acts—Riemer holds that we encounter another basic element of Madison's philosophy: "When the safety and happiness of the people were at stake, Madison's usually high regard for existing legal forms would give way to his insistence upon placing the substance of republican liberty and union ahead of accepted but malfunctioning constitutional or legal forms" (p. 136).

Riemer also explores Madison's extended-republic theory at some length. He notes certain difficulties with this theory that are of more than passing interest to the student of U.S. politics today. For instance, Madison seemed to believe that there is an objective "public interest and that it can be identified," a belief that, Riemer surmises, "is ultimately grounded in his natural law orientation" (p. 124). Yet his failure to define this central concept means, as Riemer remarks, that his theory "may not be scientifically operational in modern terms" (p. 125). And in this vein, as Riemer notes, his theory is concerned with preventing factious rule—which "limits [its] relevance . . . for those who favor a broad exercise of national power in what they hold to be the national interest" (p. 118).

While noting that Madison "was by no means a systematic, comprehensive, sophisticated student of political theory," Riemer concludes by pointing out the ways in which his thought provides "the germ of a durable, penetrating, and persuasive democratic theory for man today" (p. 158). Though his view of human nature prevented him from even contemplating a heaven on earth, Madison did repose sufficient faith in man to take the "calculated risks" (p. 164) involved in establishing an extensive republican regime under the forms of the Constitution.

GEORGE W. CAREY

Georgetown University

Self and Society: A Study in Gandhian Thought. By Ramashray Roy (Beverly Hills: Sage, 1985 [in collaboration with United Nations University, Tokyo]. 205p. $25.00).

The author maintains that the predicaments of Western modernity can be overcome through the Gandhian conception of the self and of the relationship of the self to society and nature. According to him, the pathology of modern civilization has its origin in the seventeenth century shift in European thought —and practice—from the conception of man as being an integral part of a larger order to the concept of man as an autonomous, self-defining, self-sufficient and possessive-individualistic ego. In Roy's view, such Western attempts to repair this "broken totality" as expressivism, Hegelianism, Marxism and romanticism do not lead us out of the "satanic civilization," which remains committed to "the multiplication of wants and machinery contrived to supply them." What is required, according to Gandhi, we are told, is not mere "external" or structural changes in social and political organization but the self-transformation, or swaraj, of human beings, leading to the realization of "the essential unity of God and man and for that matter of all that lives." Against the "broken totality" of modern Western civilization, Roy advances Gandhi's peculiarly Hindu philosophy of life, according to which, as noted by Gandhi, "all life (not only human beings, but all sentient beings) is one" (p. 73). Salvation, accordingly, is not to be confined to human beings; it has to extend to *all* God's creatures. This Hindu philosophy of life, Gandhi believed, excludes all exploitation. It defines salvation as the realization, by the self, of Truth, which means "not only truthfulness in word, but truthfulness in thought also, and not only the relative truth of our conception, but the absolute Truth, the Eternal Principle, that is God" (p. 71). This Hindu conception of the integral unity of all life, as interpreted by Gandhi, implies that the process of self-realization, that is, Truth realization, can only be achieved through the moral-political actions of *satyagraha* and *sarvodaya*. Satyagraha, by rejoining politics and religion, we are reminded, repairs the broken totality of modern times.

Roy succeeds in bringing out the important contrasts between the modern Western paradigm of politics and the Gandhian conception of the self, society, and nature. The treatment of the distinctive philosophical foundations of those two conceptions or models of social and political life is the chief merit of this book. It suffers, however, from the limitations of the author's self-acknowledged training as a behavioralist. The book could have gained from a greater use of the available literature on Indian philosophy and on Gandhian thought. While it is indeed true that Gandhian thought has its roots in Hindu philosophy, it has also to be recognized that the former was influenced considerably by such strands of non-Hindu thought as Tolstoy, Ruskin, Thoreau, the Bible, and British jurisprudence.

While I would still regard the book as giving important and useful insights into Gandhi's philosophical undermining of the technological or scientistic model of modern Western politics, I am somewhat disappointed by the author's inadequate treatment of Gandhi's satyagraha mode of liberative political action. How satyagraha constitutes a realistic and effective mode of emancipatory action against the technological model of politics remains to be demonstrated. From the author's treatment of the subject, I am left to wonder if Gandhi's alternative philosophy of the self and society is simply an utopian blueprint. Also left unresolved is the question of whether or not the Gandhian satyagraha is merely a voluntarist conception of social change.

It seems to me that for a proper understanding of the distinctiveness and relevance of Gandhi's philosophy and method of liberative action by those who suffer from the predicaments of "modern industrial civilization," it is necessary to compare and contrast the Gandhian "project" with both liberalism and Marxism. This has not been the specific objective of Ramashray Roy, but his book will be of great help to anyone undertaking such a larger study.

THOMAS PANTHAM

Maharaja Sayajirao University, Baroda

The Evolution of Rights in Liberal Theory. By Ian Shapiro (Cambridge: Cambridge University Press, 1986. x, 326p. $39.50, cloth; $11.95, paper).

Ian Shapiro's book is both more and less ambitious than its title suggests. It is more ambitious because Professor Shapiro's concern is not only to trace the evolution of liberal theories of rights, but to reveal the ideological force of such theories. As he says in the concluding chapter, "My aim has been to show . . . how conventional liberal values have come to function, and continue to function, conservatively in today's world" (p. 304). As an account of the *evolution* of right in liberal theory, however, it is less ambitious than the title suggests, for Professor Shapiro concentrates his attention on only four "moments" in the development of liberalism, "moments" represented by Thomas Hobbes, John Locke, Robert Nozick, and John Rawls.

Indeed, the author proceeds by fours in this book. In the first chapter he states that a "claim about rights generally involves a fourfold assertion about the *subject* of entitlement, the *substance* of entitlement, the *basis* for entitlement, and the *purpose* of entitlement" (p. 14, Shapiro's emphasis). In the succeeding chapters on the four "moments," then, Professor Shapiro analyzes the theories of Hobbes, Locke, Nozick, and Rawls in terms of their conceptions of the subject, the substance, the basis, and the purpose of rights. In each case, he follows this analysis with an account of the implications—intended or not—of the philosopher's theory for the development of liberalism as an ideology.

These accounts of ideological implications are to my mind the most interesting parts of a most interesting book. According to Shapiro, for instance, Hobbes's political absolutism was due largely to the fact that he was caught in the "transitional moment" between feudalism and capitalism, a time when the economic and social practices necessary to the survival of the "negative-libertarian" society Hobbes really favored were neither fully nor firmly established. In these circumstances, Shapiro argues, Hobbes had to insist on the need for a "decisive authoritarian power" to enforce "the rules and norms essential for social action" (p. 66). Later, however, when the necessary practices and institutions had come to seem part of the natural order of things, the implications of Hobbes's arguments pointed away from absolutism and toward liberalism. Thus Hobbes's identification of "natural man" with "private man" led him to conclude that the purpose of a public sphere "was merely to facilitate private interaction. This is a move of immense ideological significance because it provides for the germination of the liberal notion that the state . . . is fundamentally undesirable, an illegitimate intruder except where its actions facilitate private interaction" (p. 60).

With regard to Locke, the representative of "the classical moment" of liberalism, intentions and implications again point in somewhat different directions. In arguing for toleration and against conformity in religious matters, for instance, Locke conceived of rights themselves "primarily in terms of toleration, of the spheres of people's lives in which they may be left alone" (p. 117). Such a conception leaves little room for a "doctrine of democratic participation," Shapiro says, and once religious toleration and conformity were no longer at the center of controversy, "the negative libertarian view of freedom Locke embraced had vastly different and more conservative ideological implications" (p. 117).

Both Hobbes and Locke, then, contributed by implication to the "negative-libertarian" view of rights and society—a view shared, the author says, by Nozick and Rawls, representatives respectively of the "neoclassical" and "Keynesian" moments. The chief difference between the two on Shapiro's analysis is that Nozick advocates the "minimal state" because he clings to the equilibrium theory of the self-regulating market while Rawls, who follows Keynes in rejecting this theory, advocates "the liberal corporatist state" (pp. 262-64). This is the only significant difference between them, though, and in the end we find that they join Hobbes and Locke in regarding "the purpose of political organization generally, and the civil rights constituted by it in particular, as being to preserve a negative libertarian society in which individuals can pursue their different individual goals unimpeded" (p. 283). If one is tempted to say that a political organization ought to serve this purpose, Shapiro is ready to point out that such a vision of politics and rights denigrates political participation and the public sphere, fails to foster a sense of community, and succeeds all too well in legitimizing the dominant interests in capitalist societies.

Professor Shapiro is not the first to bring these charges against liberalism, of course, and perhaps that is why he does not pursue them here. He sees his task, instead, as "historical anthropology" (p. 303), with the particular aim of providing "an evolutionary anthropology of establishment values" (p. 8). Does he succeed in this endeavor? He does if one is willing to count well-informed, closely argued, and provocative readings of Hobbes, Locke, Nozick, and Rawls as the full measure of success. Certainly it is a triumph of sorts for Shapiro to find something fresh and interesting to say about each of these thoroughly scrutinized philosophers. If one takes his notion of an "evolutionary anthropology" seriously, however, then one must conclude that there are some links missing from *The Evolution of Rights in Liberal Theory.*

This is most evident when one considers that Shapiro devotes his attention to two English philosophers of the seventeenth, and two U.S. philosophers of the twentieth, century. Between his pairs of subjects stretches more than 250 years of ideological evolution of which we are told almost nothing. To be sure, Shapiro does refer several times to the importance of the rise of the concept of equilibrium in the eighteenth century, and Bentham, Mill, and Sidgwick do appear briefly, but there is no attempt to trace shifting conceptions of rights or developments in liberalism through the eighteenth and nineteenth centuries. Nor is there any attempt to explain why so many influential philosophers of the nineteenth century, both within and without the Anglo-American tradition, were suspicious of, or even hostile to, the natural-rights theories associated with Hobbes and Locke. A second kind of missing link is more a matter of method than content. Through the use of such evolutionary metaphors as "lethal mutations" (p. 5) Professor Shapiro suggests that the fate of a theory rests in large part on its ability to adapt —or, more precisely, to prove adaptable—to a changing environment. Hobbes did not intend to justify the capitalist market, for instance, but his "negative libertarianism survived and achieved the preeminent status it did in the dominant ideology because of its affinity with these emerging economic and social relations" (p. 63). Clearly there is some relationship between ideas and institutions here (or *superstructure* and *structure* in another idiom) but what is it? Shapiro rejects any sort of simple deterministic explanation of the relationship, certainly, but aside from invoking the evolutionary metaphor, there is no clue as to what he would put in its place.

There is only so much that one can do in a book, however, and I am inclined to think that Ian Shapiro has done quite a lot in this one. There is much to admire in *The Evolution of Rights in Liberal Theory,* and much to argue with, too, which is one of the reasons for admiring it. Professor Shapiro writes clearly, argues vigorously, and, for an author intent on unmasking the ideological functions of rights theories, remains remarkably free himself of ideological dogma. If he does not tell us all we might wish to know in this book, then we may well wish that he will tell us more in another.

RICHARD DAGGER

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The Longing for Total Revolution: Philosophic Sources of Social Discontent from Rousseau to Marx and Nietzsche. By Bernard Yack (Princeton: Princeton University Press, 1986. xvii, 390p. $35.00).

Bernard Yack has both historical and critical projects in this book. The historical project locates the heritage of German idealism within revolutionary thought from Schiller and the Left Hegelians through Marx and Nietzsche. These thinkers are often viewed as inheriting and secularizing Christian ideals. Yack argues, however, that their ideals are distinctively modern, reflecting notions of human dignity and freedom introduced by Rousseau and philosophically expressed by Kant. According to Kant, human dignity stems from capacities for freely determined rational judgments, that is, judgments not determined by natural contingencies. Yack is especially concerned with the social conclusions various thinkers drew from this polarity of freedom and determination. Kant and Hegel saw in it a more or less inevitable conflict between inner freedom and social contingency. In contrast, their leftist successors held that freedom should and could be realized within social relations. To the extent that social relations do not express freedom, they are dehumanizing and ought to be transformed.

Yack's historical treatment of this theme is wide ranging, scholarly, fluid, and often insightful. His view that German revolutionary thought can be seen as providing answers to "left Kantian" questions is an extremely useful context of interpretation. The book's critical project, however, will prove controversial. Yack's targets are Marx and Nietzsche, who, he argues, both fail to understand that extending Kant's notion of freedom to social relations is self-contradictory. For Kant, freedom is an inner, "noumenal" quality. Social existence is "phenomenal," and hence subject to causal contingencies. By demanding that human dignity qua freedom be realized within economic and cultural relations, Marx and Nietzsche misunderstand the possibility of extending Kantian conceptions of freedom to the realm of natural contingencies. Because they retain Kant's conception of human dignity while rejecting its dualism, they fall into self-contradiction. This in turn reflects Marx's and Nietzsche's failure to understand that social contingency will always exist, and that therefore "total revolution" will always fail. Hegel, on the other hand, recognized these limits to freedom, and was therefore able to conceive of a society in which inner freedom and external social contingencies might coexist. Thus Yack's philosophic aim is to show that a Hegelian critique of Marx and Nietzsche is more plausible than their own critiques of Hegel. His political aim is to show that revolutionary demands are misplaced, the product of wishful thinking based on self-contradictory premises.

Perhaps because Yack presents his critical thesis in genetic terms—through a history of ideas—it lacks the philosophic argument one might wish, especially given its sweeping qualities. Key arguments are often presented as if they were self-evident. For example, the most important claim for Yack's thesis is that Kant understood that inner freedom could not be expressed in social relations (Chap. 3). Yack asserts that the "left Kantian" demand that freedom be realized in social relations rests on a very obvious category error: the noumenal realm of freedom is incommensurable with the phenomenal world of social relations. But Yack surely misses the form of Kant's questioning. Kant assumed that moral and rational experiences exist and that they already have an imperfect reality in the social world. His critical thought had to do with identifying what we must presuppose (such as our freedom) for our moral experiences to be intelligible. The duality is an epistemological, not a cosmological, one. This makes a difference for political conclusions: although, as Yack points out, Kant was no revolutionary, he did see reforms toward legal, parliamentary regimes as conditions of extending and universalizing moral experience (see *The Metaphysics of Morals*). He even had a theory of the transformation process (see his *Idea for a Universal History*). Without the benefit of argument, it is difficult to see how Yack can sever Kant's notion of inner freedom so cleanly from social relations. Yet this assumption is what allows him to take for granted that left-Kantian demands for realizing freedom in social relations are misplaced.

A second limitation is that too much of the argument is carried by rhetorical attributions of totalistic goals. Of Marx and Nietzsche, for example, Yack writes that "by providing a single focus for all our dissatisfaction with the world, the longing for total revolution raises great hopes for the transfiguration, however defined, of the human condition" (p. 368). There is certainly plenty in Marx and Nietzsche to support Yack's many statements to this effect. But there is also much more. Because he carries out his own analysis in such sweeping terms, one feels Yack has missed the often finely textured and subtle ways Marx and Nietzsche deal with the problems he so often acutely identifies.

Finally, Yack's project itself is likely to be controversial. By focusing on "philosophical sources of social discontent," he in effect brackets what many will see as equally important issues, namely, whether Marx, Nietzsche, and others provide new insights into rapidly changing institutions. Yack is certainly correct in arguing that demands for change can result from new ideas about what is desirable and about what can and cannot be changed. And he claims that his type of analysis does not preclude others (e.g., p. 254). Nonetheless, Yack's critical conclusions are hard to separate from his approach. Because he investigates only conceptual sources of discontent—in isolation from both practices and institutional contexts —he adopts a perspective in which the individual is simply a posited internal conceptual state, whose qualities are explained only by inherited ideas. Likewise, institutions appear as undifferentiated, external "obstacles" to individual demands. Yack continually writes of institutions only in the abstract, as if they had the same status as any natural contingency in terms of their independence of human will. In spite of Yack's protestations to the contrary (p. 368-69), one cannot stay within his project and avoid the neoconservative conclusion that the best solutions to social problems are lowered expectations.

MARK WARREN

Northwestern University

AMERICAN POLITICS

Justice Downwind: America's Atomic Testing Program in the 1950s. By Howard Ball (New York: Oxford University Press, 1986. xviii, 280p. $21.95).

The Whale and the Reactor: A Search for Limits in an Age of High Technology. By Langdon Winner (Chicago: University of Chicago Press, 1986. xiv, 200p. $17.50).

The study of the social, economic, and political impacts of technology upon human society and its institutions has attracted the attention of numerous scholars in recent decades. Faith in the utopian conviction that technological innovation would lead to previously unimaginable improvements in the quality of life has been replaced by skepticism and ambivalence. The two volumes reviewed here use very different approaches to explore the unintended effects of new technologies.

Howard Ball's *Justice Downwind* is an absorbing case study of the aftermath of atomic-weapons tests conducted by the U.S. Atomic Energy Commission (AEC) at its Nevada test site between 1951 and 1958. In the Cold War atmosphere of the 1950s, the goal of maintaining U.S. nuclear superiority over the Soviet Union took precedence over the AEC's obligations to protect thousands of residents near the test site from radioactive fallout. While the patriotic, fervently anti-Communist Mormons of Southern Utah initially viewed the tests as necessary for national defense, such acceptance gradually gave way to a growing uneasiness. The AEC responded with public relations gestures and suppression of research data that cast doubt on its public reassurances. Increasingly cases of leukemia in areas directly in the path of the fallout eventually led a group of "downwinders" to sue for damages in a federal district court in 1979. Ball does a masterful job of discussing the legal obstacles faced by the plaintiffs under the 1946 Federal Tort Claims Act, the strategies and arguments used by attorneys for both sides, and the judge's 1984 ruling that AEC negligence was "more probably than not" responsible for the injuries of 10 of the 24 plaintiffs (p. 138). He argues, however, that judicial remedies may not be the most appropriate ones in such cases because of the time required for litigation and the difficulty of establishing causation in situations where the adverse health effects of toxic substances may not appear until many years after exposure. A better remedy, in Ball's view, would be some form of legislative compensation. Efforts in that direction, however, have been stalled by the uncertain costs involved, congressional indifference, and opposition from the nuclear industry, insurance companies, and executive-branch agencies.

Justice Downwind's strongest points are its solid historical documentation and clear explanation of relevant points of law and bureaucratic politics. Several chapters are marred, however, by repetitious writing that could have been made leaner and more concise by better editing.

More ambitious in its aims and more theoretical in its approach, Langdon Winner's *The Whale and the Reactor* is a call to develop a "political philosophy of technology": "Just as Plato and Aristotle posed the question, What is the best form of political society? so also an age of high technology ought to ask, What forms of technology are compatible with the kind of society we want to build?" (p. 52). The scientists, engineers, bureaucrats, business executives, and politicians who make technological choices rarely pause to consider what values or ends are served by the technological marvels they create. Winner contends that certain technologies "constitute political phenomena in their own right" (p. 21) because their use encourages or requires particular kinds of political relationships—"centralized or decentralized, egalitarian or inegalitarian, repressive or liberating" (p. 29). He discourses on a wide variety of topics, ranging from the computer "revolution" to the politics of language and differing conceptions of "nature" in the writings of economists, philosophers, and ecologists. Conventional approaches to understanding technology, such as those offered by risk assessment, technological determinism, "appropriate" technology, Marxism, liberalism, or technology assessment, are seen as inadequate foundations on which to construct a philosophy of technology because they fail to ask or answer the most profound questions concerning the social and political ends of technological developments.

The Whale and the Reactor is more readable, coherently argued, and accessible to a wider audience than the author's earlier *Autonomous Technology* (1977). Its greatest strengths are its elegant writing and reasoning and its skillful exposition of the broader issues lying beneath the surface of contemporary discussions of technology. The book offers only vague generalities, however, in tackling the problem of reconciling technical expertise and political democracy. While Winner suggests that technocrats and ordinary citizens alike join in a dialogue to discover which technologies will best promote social justice, equality, and other important political values, he acknowledges that such an enterprise would require "qualities of judiciousness in the populace" that are now rare, as well as the radical transformation of many existing institutions (p. 55). The reader is left wondering about the feasibility and effectiveness of such proposed solutions to the problems he rightly deplores.

The flaws in both books are minor, however, and both are valuable additions to the literature on science and technology policy. Students of bureaucracy and the law of torts may find the Ball book of particular interest, while those inclined to ponder deeper and more fundamental questions concerning relationships between humans and machines may turn to Winner.

JAMES R. TEMPLES

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Science, Technology, and Public Policy. By Richard Barke (Washington: Congressional Quarterly, 1986. ix, 245p. $10.95, paper).

Science and technology policy, as a field, confronts two large problems. First, the subject matter is exceptionally complex and wide-ranging. While there are discrete policies for science and technology, there are also many less-clear relations between science and technology and nonscience policies. This is because science and technology is a part—generally the "front-end" or new part—of most other policy functions, such as defense, energy, transportation, and so on. Second, the field is inherently fast moving, a fact that means that the subject matter changes so quickly that books are soon outdated.

Hence, those who teach in the field have difficulty finding readings that overcome these problems. There is a need for material that covers a great deal of territory and at the same time uses illustrative examples that are timely.

Richard Barke's *Science, Technology, and Public Policy* attempts to address these issues. It is successful in that it has much fresh information which will make it especially helpful in teaching. It is not as successful in aggregating and synthesizing the disparate elements of the field.

Barke uses a framework of four types of constraints to investigate general characteristics of all science and technology policies. These are law, knowledge, coordination, and politics. Using this framework, Barke essentially takes the reader through the interactions of science and technology with various institutions of U.S. government (president, Congress, bureaucracy, courts). He also discusses the public and scientists as participants in the science-and-technology policy process. He includes chapter-length case studies of science in policy (hazardous waste) and policy for science (the space telescope). A concluding chapter sums up the author's view of the problems associated with science-and-technology policy and of what, if anything, can be done about them.

What is wrong appears to be a problem specific less to science-and-technology policy than to contradictions in democratic government generally. While Barke does not propose any grand reforms, he does say that whatever reforms in the system are considered must deal with the sheer "complexity of the science and technology policy process." This complexity is manifested in three ways: "fragmentation of decision making, the inseparability of facts and values, and the balancing of considerations" (p. 213).

Barke has written a useful book. However, his framework does not really bring about the coherence he surely sought. Each chapter contains a sequence of topics that do not always flow together. There is a certain choppiness, and this characteristic is exacerbated by an overuse of quick examples.

The author seems aware that the chapters contain a lot of trees in search of a forest. He points out that he hopes the two cases at the end of the book will help the reader "to see how the pieces fit" (p. vi). The cases do help. But they are late in the book, and might have been integrated with earlier material to advantage. Hence, the framework of constraints is only a partial solution to the dilemma cited earlier, namely, a field that is extremely disparate and wide-ranging.

These reservations notwithstanding, I would expect many teachers of science-and-technology policy courses to adopt this book. It helps to fill a genuine need. It is up-to-date and interesting—and its very limitations in conceptual approach may stimulate others to try alternative methods. The challenge is large but, as Barke has shown, involves a subject that is worth the effort.

W. HENRY LAMBRIGHT

Syracuse University

American Women and Political Participation: The Impacts of Work, Generation, and Feminism. By Karen Beckwith (Westport, CT: Greenwood, 1986. xiv, 185p. $29.95).

Karen Beckwith has written a straightforward and useful book that uses mass-level data from the Michigan presidential election-year surveys to explore the extent and roots of gender differences in political participation over the period from 1952 to 1976. She proceeds thoroughly and systematically to investigate not only the bivariate relationship between gender and political activity but also the effects upon political involvement of several variables—among them, work-force participation; membership in a cohort that comes of age in a decade of feminist protest; commitment to a feminist ideology; class; and race.

The major findings are hardly astonishing. First, with the single exception of the greater likelihood that women will agree that politics is too complicated to be comprehensible to the average person, any differences between men and women are minimal. Second, employment status, political generation, and feminist ideology are less potent predictors of political activity than is socioeconomic status. Along the way, various interesting tidbits appear—such as the fact that the subjective evaluations of respondent's level of political information made by interviewers (which give men the edge) are not borne out by the actual replies to questions testing political knowledge (which show no consistent relationship between gender and information).

Both the data and the analysis have some weaknesses that bear mention, however. First, as Beckwith herself points out, she is confined by the limitations of the National Election Study—which, given its emphasis upon electoral activity, is a less than perfect source for a study of the multiple ways in which U.S. citizens take part in politics. In addition, the data are a bit stale. Beckwith argues that "by ending our examination of women's political participation in 1976, we allow ourselves the advantage of avoiding a consideration of the rise of an intense, antifeminist minority and the confounding effects of the conservative Reagan administration and its opposition to women's rights" (p. 5). However, this seems to be making virtue of convenience, especially when recent Michigan surveys contain many items of relevance to her subject.

With respect to data analysis, there is a problem with data reduction. Even when Beckwith creates several activity scales (some of which, of necessity, mix attitudinal items with measures of political participation), there are six indicators of political activity and seven presidential-election years. Naturally, the results for 42 data points contain lots of noise. Happily, Beckwith eschews the temptation to invent an explanation for each random blip in the data. However, the end result is a certain mushiness. Finally the great care with which the analysis proceeds—on occasion the book's greatest asset—is something of a mixed blessing. For example, Beckwith devotes several pages (pp. 35-39) to a well-documented discussion of women's employment. Taken on its own terms, the argument proceeds quite reasonably. However, readers of this volume probably do not need to spend several pages learning that women have traditionally taken responsibility for child care and household management or that they are paid less than men when they enter the work force.

In spite of these shortcomings, Karen Beckwith has given us a sensible and useful volume that makes an addition to the library of anyone concerned with citizen political behavior or women in politics.

KAY LEHMAN SCHLOZMAN

Boston College

Administering the New Federalism. Edited by Lewis G. Bender and James A. Stever (Boulder, CO: Westview, 1986. ix, 369p. $39.95).

This is an edited book which seeks to examine the implications of President Reagan's domestic program, called the New Federalism, for state and local governments and for selected policy and management issues. For the most part, the focus is on the first four years of the Reagan administration, although some attention is given to the more recent period. In the introduction, editors Lewis Bender and James Stever attempt to draw some parallels between large organizations and the U.S. federal system. They argue that many administrative federal issues are but a subset of similar issues facing any large organization. These include those dealing with questions of leadership, coordination, regulation and accountability, authority, and efficiency. The book's first section features articles by Timothy Conlan and Richard Williamson who discuss the origins of the New Federalism agenda and some of the competing values apparent in the Reagan program.

Section 2 focuses on the implications of New Federalism for state and local governments. Irene Rothenberg and George Gordon in their chapter compare some of the administrative guidelines issued during the Reagan years with those of previous presidencies. They conclude that the major effect of New Federalism for local governments "has been to interject another government between themselves and funding agencies" (p. 90). In one of the book's best chapters, Paul Posner argues that our traditional distinctions between categorical and block grants may no longer be as useful as other methods of classifying grant programs. He suggests degree of state-management authority to be the most important variable. In an equally reflective article, Susan MacManus, Robert Stein, and V. Howard Savage assess the impacts of New Federalism for Texas cities in the 1981–1983 period, focusing especially on Houston. They find little "short-term" impact of the Reagan initiatives, citing Texas's changing economic and demographic conditions as more important. Focusing her essay on small cities in North Carolina, Beverly Cigler finds by contrast, "major impacts on North Carolina's small communities, influencing their ability to undertake their responsibilities" (p. 176). Examining the situation in Tulsa, Raymond Rosenfeld and Alan Frankle in their chapter suggest that the potential hardships of the New Federalism agenda prompted Tulsa voters to be more supportive of local taxes and higher service fee, thus blunting some of the negative aspects of the administration's program.

Part 3 looks at the implications of New Federalism in various policy areas. Charles Moore and David Sink examine housing policy in the Reagan years with special attention given to Birmingham, Alabama. They find the situation in Birmingham to be "grim" (p. 221). The Reagan program, they say, is contributing to a "halt of federal efforts to provide additional decent housing for those residents who cannot secure it on their own" (p. 221). Pinky Wassenberg examines state response to reductions of federal funds for water-pollution control programs and the administration's attempt to return responsibility for establishing and enforcing pollution control standards to the states. The result, Wassenberg argues, is to deprive states "of the increased federal assistance they [need] to increase the administrative and technical capacity of state programs" (p. 243). Dale Krane in his article examines the Community Development Block Grant (CDBG) program in Mississippi and state takeover of that program resulting from the Reagan initiatives. He concludes that earlier "fears" of state takeover "appear to be unfounded. . . . Mississippi's track record to date is positive" (pp. 265–66). In a similar vein, Susan MacManus finds little negative impact of CDBG restructuring in Houston. By contrast, she finds the new guidelines "offered the new mayor and the CD director the opportunity to get the program under control" (p. 289).

The final two chapters of the book offer something of a point-counterpoint exchange concerning the merits of New Federalism. Robert Agranoff and Valerie Rinkle suggest that New Federalism may represent a major opportunity for opening new channels of communication—and thus new structures for planning and problem solving—between state and local governments. David Walker, in the book's concluding chapter, observes that the weakened nature of state governments in the U.S. federal system poses significant obstacles to any domestic program that rests for its success largely on the will and the capacity of subnational governments for significant policy innovation and implementation.

One of this book's strengths is also one of its weaknesses. To their credit, the editors attempt to cover the full spectrum of issues surrounding the New Federalism program, including its history, philosophical background, administrative and programmatic impact, policy implications, and historic role in the U.S. federal system. Students will appreciate having one volume that attempts to cover all these topics; but, at the same time, the book cannot give adequate and complete attention to all these important issues. Perhaps a focus on only one or two major issues would have been advisable.

Further, the provocative framework established by the editors in their opening chapter is virtually ignored in the remaining chapters. The reader is left to draw his or her own conclusion as to how the points made by contributors relate or do not relate to the editors' opening arguments. Finally, the book would benefit considerably from a concluding chapter drawing together the various themes and findings suggested in the previous chapters. Here the editors might have discussed some of the contradictory findings of contributing authors, provided an assessment of New Federalism in all areas covered (is New Federalism working or not?), and might have placed these findings in the context of the themes they assert at the outset.

In spite of these limitations—and the problems apparent here do not differ markedly from those in other studies of the New Federalism—this will be accepted as a valuable addition to the growing literature assessing the implications of the Reagan presidency for domestic policy and politics. The editors and contributors bring us "up-to-date" on current assessments of New Federalism and, while those assessments ultimately may change, this volume has considerably advanced both for the lay and the scholarly reader our understanding of the Reagan years.

RICHARD L. COLE

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Intergovernmental Relations and Public Policy. Edited by J. Edwin Benton and David R. Morgan (Westport, CT: Greenwood, 1986. viii, 224p. $35.00).

With the editors' assertion that "a wedding of IGR [intergovernmental relations] and public policy was inevitable" (p. 5), I tend to agree. Certainly it is an appropriate focus for an anthology. Unfortunately, it apparently proved easier to assert the theme than to find authors with ready papers truly combining the two elements. In consequence, this volume gives us several competent pieces on IGR as such (e.g., on the Berger court's view of IGR and on metropolitan regional associations), but rather fewer that really tie the two together.

Among those that impressed me (such judgments always involve partly subjective criteria of interest) are Thompson and Scicchitano's comparing state with federal Occupational Safety and Health Administration enforcement in a seven-year period ending in 1983; Hedge and Menzel's similar focus, but in the Surface Mining program; Ervin and Watson's on state-local patterns in Community Development Grant administration; and one on changes in fiscal centralization in the states, 1957–83, by Stonecash. Each of these substantially and successfully blends the two foci of substance and process.

Other topics covered include federal-aid cutbacks, environmental policy, a comparative piece on local finance in West Germany, and an interesting brief look into the crystal ball by Beam and Benton. This last has a couple of thoughtfully provocative pages (pp. 209–10) on a reverse twist: "the localization of national programs."

It should be noted that earlier versions of one-third of the papers in this volume appeared in *Policy Studies Journal* (vol. 13 [March 1985]).

Stimulating as some of the individual empirical papers and essays are, the thought occurs to me that anthologies need more than a common nominal topic to constitute coherent volumes. If editors can arrange for authors to work within some common framework of hypotheses to be addressed, of method, or of values to be explored, we readers would be most grateful. In the book covered here, the closest to a common thread is that most pieces use statistical analysis. Most are also stronger in data than in suggesting ideas and propositions about the nature of the reciprocal relationship between the two variables contained in the volume's title.

MICHAEL D. REAGAN

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Re-electing the Governor: The 1982 Elections. Edited by Thad L. Beyle (Lanham, MD: University Press of America, 1986. xli, 336p. $15.75, paper).

Re-electing the Governor: The 1982 Elections is the result of a larger effort to understand the determinants and consequences of gubernatorial elections. In 1982, a network of state analysts was established to study two different phenomena: in the 36 states holding gubernatorial elections the successful reelection of incumbent governors and, in cases where power changed hands, the gubernatorial transition process. This volume, edited by Thad Beyle, focuses on the reelections of 16 of the 19 governors who were successful in their bids for another term. The book begins with an introductory chapter by Beyle that quite nicely summarizes some of the important aspects of gubernatorial incumbency, both in elections and in the operation of government. This introduction is followed by two interesting chapters on the role of money in gubernatorial elections, the first written by Malcolm Jewell and the second, again, by Beyle. Unfortunately the book goes downhill rapidly from this point. What comes next are 16 separate state "analyses" of the reelection triumphs of incumbent governors in 1982.

I have serious reservations concerning the utility of this book for social scientists interested in the determinants of gubernatorial election outcomes. The strategy of inquiry pursued in this book is not well suited to answering this or any other general research question. The 16 state studies are almost without exception simply descriptive accounts of the political environment surrounding the election, the election itself, and the political changes that followed the reelection. The one exception to this approach is the chapter by McDonald and Morgan, which complements the descriptive account of the reelection of Oklahoma's George Nigh with a county-level analysis of socioeconomic and political variables as well as some descriptive evidence from survey data. Even if all other chapters had pursued this type of within-state systematic analysis, the book would still come up short. The problem with this book is not just its descriptive nature (although this is certainly a major problem) but also its lack of a comparative focus. Instead of coming away from this book with a general explanation of successful gubernatorial reelection bids in 1982, what the reader gets is 16 separate descriptions of gubernatorial elections.

Even if such idiosyncratic descriptions could be melded into a more general explanation it would be of little utility due to problems of case selection. This book includes "analyses" of only 16 of the 19 successful bids for reelection. The problem here is not just that three successful reelection bids are excluded but also that the six unsuccessful bids and the eleven cases where no incumbent ran are overlooked. To explain successful gubernatorial reelection bids, you cannot limit yourself to examining only those cases where governors are reelected. Surely the different conditions surrounding successful and unsuccessful bids for reelection, as well as the races not involving an incumbent, are important to an explanation of reelection successes. In short, to explain successful bids for gubernatorial reelection adequately you must be able to explain the determinants of gubernatorial elections in general. This is not accomplished by relying on descriptive accounts of a subset of elections "analyzed" one at a time.

The idea of postelection studies has a good deal of merit. My recommendation, however, is that the methodology be comparative and systematic rather than idiosyncratic and descriptive. This book is of the latter type and, therefore, is of little utility in a search for answers to general questions such as why some incumbent governors are successful in their bids for reelection.

As a final matter, I feel compelled to comment on the poor quality of craftsmanship on the part of the publisher. The most glaring example of this is that the footnotes at the end of the introductory chapter bear no relationship whatsoever to the footnote marks in the text. As near as I can determine, the footnotes at the end of the chapter belong at the end of a chapter from the companion volume on gubernatorial transition. Other examples are of the more nitpicky variety such as misspellings, and asterisks in the tables with no corresponding explanation.

THOMAS M. HOLBROOK

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Presidential Transitions: Eisenhower through Reagan. By Carl M. Brauer (New York: Oxford University Press, 1986. xvii, 310p. $22.95).

Presidential transitions, especially since receiving federal subsidies in 1968, have become something of a growth industry. Eisenhower's transition team was small, consisting of a few intimates; Reagan's, by contrast, was an elaborate effort with over one thousand salaried staff and numerous volunteers. There has, however, been no comprehensive book on the topic since Laurin Henry's *Presidential Transitions*, which appeared in 1960. Historian Carl Brauer, currently associated with the Center for Business and Government at the JFK School, has undertaken to update the story.

Brauer writes for a general, rather than scholarly, audience as he examines the basic dynamics of the Eisenhower, Kennedy, Nixon, Carter, and Reagan transitions. His treatment is broader than just the interregnum period (from election to inauguration) and spans the time from preelection transition planning to "established incumbency"—generally through the first year of the administration.

Each chapter-length case study of the five transitions explores a variety of themes, including the new president's interpretation of his mandate, his relations with the outgoing president, his stance vis-à-vis the Congress and the development of a legislative program, his relationship with the media, his appointment of White House office staff and cabinet and subcabinet officials, and a *tour d'horizon* of the various policy issues—foreign, military, domestic, and economic—that faced his administration.

Brauer's research is thorough, relying on memoirs, interviews with key participants, secondary reportage, and, in the cases of Eisenhower and Kennedy, presidential archives. The lack of archival material pertaining to the three most recent presidencies, though unavoidable, is regrettable. As scholars of the presidency know well, there are often considerable discrepancies between what is remembered by participants—who, as in the case of the Nixon administration, may have an ax to grind—and what is demonstrated by the historical record. (As Brauer volunteers, this renders his discussion, especially of the Nixon transition, "relatively tentative and speculative" (p. 122). Also disturbing is Brauer's apparent neglect of standard comparative analyses of several of his major themes, for example Calvin MacKenzie's work on presidential appointments (*The Politics of Presidential Appointments* [New York: Free Press, 1981]) and Richard T. Johnson's on White House organization (*Managing the White House* [New York: Harper & Row, 1974]). The result is a treatment which, at least for political scientists, is rather long on description and short on comparative analysis.

Brauer does extract some salient patterns from the transitions he studied. First, he finds, newly elected presidents fail to learn from the mistakes of their predecessors. They pay little heed to the incumbent's advice, for the latter, after all, has just been repudiated at the polls. In the postelection euphoria, "hope, sometimes arrogance, hubris, and a sense of infallibility run high. . . . This atmosphere . . . largely explains why new Presidents have been unable or unwilling to learn from their predecessors" (p. 258). Moreover, new presidents tend to overreact to perceived flaws in the man and administration they follow. Ike was too antipolitical, Kennedy too antiorganizational, Carter too antiimperial.

Second, new presidents are overly prone to become enamored of organizational reform and to bow to the false God of "cabinet government." All save Kennedy entered office proclaiming the virtues of the cabinet and underestimating the importance of the White House staff. Later, they were forced to recant.

Third, in spite of campaign rhetoric promising dramatic changes in style and policy, basic continuity between one administration and the next, Brauer argues, is the rule rather than the exception. Real policy change is basically incremental, occurring over the life of several administrations. "Symbol and rhetoric tend to change much more than does the substance of policy" (p. 266).

Lastly, and somewhat ironically, the caliber of the transition itself—though it may help the new president hit the ground running—may not have all that much influence on the overall quality of the administration. Carter's transition effort, which he subsidized with his own campaign funds, was among the most comprehensive and carefully planned. Yet in Brauer's view, Carter's administration was among the least successful. In spite of federal funding for transition efforts, Brauer observes, "a newly elected President's most fundamental problems are far more attributable to his own shortcomings" (p. 183).

Brauer makes several suggestions he urges new presidents to heed. Noting that Carter, a stranger to Washington, surrounded himself with Georgians and that Reagan, unfamiliar with foreign policy, made little effort to remedy his ignorance, Brauer counsels the victors to "take a good hard look at themselves and determine how to compensate for their weaknesses—their strengths just naturally assert themselves" (p. 257). They should also examine more closely the errors made by their predecessors, give up utopian notions about cabinet government, pay closer attention to selecting their senior White House aides and subcabinet appointees, and stop bashing the career bureaucracy—with whom they will need to establish effective working relationships.

It is much easier to criticize books than to write them. I wish, however, that Brauer had spent more time on the organizational detail and dynamics of the crucial interregnum period. Further, though he alludes to the influence of the president's personality and beliefs on his transition and early incumbency, this theme remains largely undeveloped. In spite of its shortcomings, however, Brauer's study is informative, well written, and a useful addition to the literature.

RICHARD L. SCHOTT

University of Texas, Austin

Managing the Presidency: Carter, Reagan, and the Search for Executive Harmony. By Colin Campbell (Pittsburgh: University of Pittsburgh Press, 1986. xx, 310p. $19.95).

Colin Campbell's major new book concerns itself with the institutionalized presidency, the performance of presidents, and presidential underutilization of the state apparatus. Campbell feels that writers on the presidency have created a false dichotomy between bureaucratic neutral competence and responsiveness. To Campbell, neutral competence need not lead to bureaucratic insularity and lack of responsiveness to the president. Neither does emphasis on creating a bureaucracy responsive to presidents mean that it will be incompetent. Rather, Campbell suggests that a mix between the two is possible and desirable, a situation that he terms "policy competence." Campbell's theory, then, is built on the interaction between neutral competence and responsive competence. A resulting typology occurs such that a bureaucracy that is both competent and responsive is the desirably "policy competent" bureaucracy. If it is competent but unresponsive, then neutral competence results; if incompetent though responsive, political incompetence arises; and lastly, if it is both incompetent and unresponsive, nonpolitical incompetence results. According to Campbell, presidents have some control over this mix.

In successive chapters, Campbell discusses the historical evolution of the cabinet, compares the U.S. cabinet experience to the British, looks at presidential organization of the White House, economic policy-making, and budgeting and management issues (chaps. 2-6). The chapters are presented as comparative case studies of the Carter and Reagan experiences, though a wealth of other materials are brought into the analysis, including British practices and presidential developments from FDR through Ford. Campbell's breadth and depth of knowledge is impressive, and he uses this knowledge to inform his case studies. Further, these case studies are presented within the analytic framework already mentioned and further developed as appropriate to each topic. As such, these chapters are not only informative but theoretical and analytical and therefore should serve as a model for the comparative-case approach.

These chapters provide a wealth of illuminating information and insights as well. Some that struck me include the illusion of decentralization and cabinet consultation in the Carter administration. Rather, Carter's penchant for detail and his reliance on his Georgian aides centralized decision making. Secondly, Reagan's mixed collegial-hierarchial approach, using the troika of Meese-Baker-Deaver early on, not only suited Reagan's personal style, but may have been an effective device to ensure both collegiality and discussion. Further, Campbell agues that economic pressures on the budget of the 1970s and 1980s, which led to budgetary retraction, have undermined the Office of Management and Budget (OMB) as much, if not more, than its politicization in the years since Nixon. When budget expansion reappears, the neutral competence and institutional strength of OMB may reassert itself.

Chapter 7 is a quantitative discussion of career bureaucrats and political appointees in the Carter and Reagan years. These are based on interviews; the interview instrument is presented in the appendix. It is to be hoped that future scholars will rely on that instrument by repeating it, expanding and refining it. Thus, Campbell's work, along with Aberbach and Rockman's and Kessel's, should be seen as a replicable data base upon which to build.

I strongly recommend this book. Though long, it is well written and well reasoned. Its theoretical arguments are strong and usually persuasive. A range of knowledge is brought to bear. Hence, this should be regarded as a model for studying the presidency. Lastly, we should thank the University of Pittsburgh Press for issuing it so economically. The book should enjoy a wide readership, one that extends beyond presidency scholars to those interested in bureaucracy, public policy, and U.S. politics as well.

JEFFREY E. COHEN

University of Illinois, Urbana

Book Reviews: American Politics

Presidents and Foreign Policy Making: From FDR to Reagan. By Cecil V. Crabb, Jr. and Kevin V. Mulcahy (Baton Rouge: Louisiana State University Press, 1986. xiv, 359p. $15.95).

Among the many points of interest in this book, there is the quite remarkable circumstance of its timing. The authors indeed could hardly have asked for a more perfect example of life imitating art. The basic purpose of their analysis is to examine "the challenge presented to the State Department's traditionally paramount position in the field of external policy by a growing number of executive departments and agencies—a phenomenon that has become pervasive and progressively acute since World War II" (p. 42). For academics and the general public alike, bemused by revelations concerning the activities of Colonel Oliver North, Admiral Poindexter, and others on the National Security Council staff in the "Irangate" affair, this book provides an intellectual and historical perspective from which to view the recent events.

The authors remark early on that the existence of the National Security Council has not really solved any of the basic problems of coherence and direction in U.S. foreign-policy making. Indeed, the NSC "may have aggravated the problem by adding another layer to an already complex foreign policy hierarchy [and] by generating confusion about who is really in charge of American foreign policy" (p. 81). To this judgment, Secretary of State George Shultz presumably could offer only a heartfelt *amen*. The basic difficulty that Crabb and Mulcahy face, however, is in offering evidence or even hope that the current disarray in U.S. policy-making might somehow be put right with appropriate changes in the institutional framework of the decisional process. The authors have done a very good job of describing the disease. Whether they may be judged equally successful in suggesting cures is another matter.

The book essentially falls into three parts: an initial overview of basic problems in the U.S. foreign-policy process, a series of case studies of various administrations from Roosevelt to Reagan, and a concluding chapter offering some potential solutions to the disarray they have so effectively described. The tale told is a familiar but still depressing one: the increasing fracturing of authority within the executive branch on foreign-policy matters, with resultant confusion and lack of direction on fundamental issues. A particular emphasis, as noted above, is on the gradual withering of the primacy of the State Department in the policy process, and the rise of competing centers of advice and even execution, particularly within the White House itself. The authors quote with approval the plaintive comment of former Senator Arthur Vandenberg that Congress could "only cooperate with one Secretary of State at a time." Unfortunately, there seems little prospect that the increasing babel of different voices on foreign policy will be replaced by a greater sense of order. Crabb and Mulcahy do offer some specific ideas for reform, such as giving the vice-president a much more substantial role in policy-making and of course restoring some of the perquisites of the State Department. They do concede that ideas for reforming the policy process have been something of a cottage industry over the last 40 years, and they are quick to admit that they offer no panacea of their own to the prevailing difficulties.

A rather depressing but perhaps also realistic conclusion from all this is that Tocqueville's famous analysis in *Democracy in America* (1835) concerning the *inherent* tendency toward confusion in democratic foreign policy may be a permanently operating factor. Certainly nothing written or done during the last several decades holds out much promise that his gloomy analysis can be refuted. The fact that this admirably researched and well-written analysis itself fails to promise any enduring solutions is perhaps the best testament to this fact.

STEPHEN A. GARRETT

Monterey Institute of International Studies

Cities of the Prairie Revisited: The Closing of the Metropolitan Frontier. By Daniel J. Elazar, with Rozann Rothman, Stephen L. Schechter, Mauren Allan Stein, and Joseph Zikmund II (Lincoln: University of Nebraska Press, 1986. 288p. $17.75).

This book examines the far-reaching effects of Lyndon Johnson's Great Society and Richard Nixon's New Federalism on local government in the United States or at least in some of the small metropolitan areas of the Midwest. The study is a second look at the metropolitan areas, first studied in 1970 by Elazar, and is one of the few comparative urban studies conducted over an extended period of time. It is contended by the authors that other studies of urban political power give only snapshots of single cities at single points in their history.

Elazar and his coauthors return after an absence of 15 years to Champaign-Urbana, Decatur, Joliet, Moline, Peoria, Rockford, Rock Island, and Springfield, Illinois; Davenport, Iowa; Duluth, Minnesota; and Pueblo, Colorado. In the first book, Elazar examined the political and social changes in these cities from the end of World War II to the beginning of the Kennedy administration. In this second visitation, an attempt is made to consider how the areas adapted to the changes brought about by the Great Society, the Vietnam War, and the Nixon years. An attempt is also made to look at the efforts of the federal and state governments to impose new and different standards upon the local governmental units. The authors claim to analyze the struggle between federalism and managerialism in the local political arenas; and they correctly assert that the cities by and large were able to assimilate external pressure and adapt it to local mores.

This book may well be the forerunner of many. It may bring to life more studies of community programs revisited after a period of years. Indeed, it may be necessary to revisit these cities of the prairie again *after* the full effects of the Reagan years have been felt upon their local revenue patterns.

The issue of continuity and change in the local power system is one of the central questions in community studies, one that the "snapshot" approach cannot answer. Examining the question over time, as is done in this study, does enable one to draw "tentative" conclusions. As the authors contend, it would appear that the more institutionalized the broad base, the more the continuity. Similarly, the least continuity is found in the activities of the "reformers" who enter for a specific change in the formal constitutional structures.

The authors make an interesting comparison between the 1972 Federal Revenue Sharing Act and the Morrill Act of 1862. Their basic argument, however, is that the solution to the real problems of U.S. cities and metropolitan areas is not in simply increasing federal largesse but in recreating the civil community.

Another contention is probably equally valid, that the problem of growth is a problem of accommodating not just numbers but newness and transience—the conditions that have always been present in the U.S. frontier.

RUSSELL M. ROSS

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Corrigible Corporations and Unruly Law. Edited by Brent Fisse and Peter A. French (San Antonio: Trinity University Press, 1985. 235p. $30.00, cloth; $14.95, paper).

This is a valuable addition to the libraries of people interested in the control of corporate behavior. Its interdisciplinary perspective could have been even more valuable with stronger contributions on a few topics. The volume contains the leading and most innovative thinking about its subject; unfortunately, it also occasionally lapses into some less well-honed analysis.

The chapters aggregate to a complete inventory of ideas for promoting compliance with law. Not surprisingly, Christopher Stone both leads off and leads with provocative proposals. He reflects on the limits of what the editors characterize as "the bold interventionist theories" he advanced in *Where the Law Ends*, and describes new strategies such as committees that would collect in-house data on problems related to company activity, problems insufficiently understood to merit a full rule promulgation by a regulatory agency. John Braithwaite suggests lodging with enforcement agencies a clearly defined set of principles to indicate who will be held responsible for specified types of noncompliance and, most radically in this country, discouraging the use of lobbyists, "professional purveyors of conflict." Fisse and French review alternatives to the traditional fine and both discuss the community service order and forced negative publicity.

The term "idea" to characterize this inventory is consciously chosen. Some of the chapters state explicitly and others suggest that much of the analysis of corporate compliance is at the level of thoughtful speculation by intelligent and experienced scholars or extrapolation from very limited data. As Fisse and French say in their useful introduction, "the interdisciplinary study of corporate activities crawls in its infancy." Braithwaite, comparing the public-interest director infavorably with his notion of creating tension in the suites and tainting the chief executive with knowledge of illegalities, recognizes that the former strategy has not been found lacking on the basis of testing and empirical work. The volume communicates this message in a less satisfying way by including a couple of chapters that seem driven by ideological conclusions and read like sermons on the evils of capitalism and bureaucracy. For example, a slap at the agency bureaucrat seems misplaced in a scholarly treatment of a complex topic. Certainly, there is variability in competence and professionalism among public employees, but that should be a starting point for an organizational analysis of compliance rather than a self-evident principle explaining law violations.

Geis's thoughtful chapter notes that even careful empirical work may not be conclusive from a policy perspective. His is a review of recent research with some surprises on who is doing the major work. His skepticism about some of the more modern approaches for studying corporations is explicit: "We will have further detailed empirical work, with its precision purchased at the price of reducing complex matters to operational terms" (p. 80). (He feels that approaches that combine legal and social-science perspectives hold promise.) The reader joins in the skepticism that stems from certain results Geis reports including the findings that "sentences are generally tougher for college-educated offenders than for persons in other categories . . . [and that] . . . the possibility of imprisonment rose with an increase in the occupational status of the offender."

Another of the book's strengths is its identification of sources of corporate noncompliance. Stone informs us that some theory holds that legislative fines may be so designed to encourage noncompliance for a greater social good. Braithwaite recognizes the importance of individuals in an arena that is often analyzed with systems ideas and abstractions. These can lead us and enforcers to forget that identifiable people occupy significant and highly influential roles in even the largest corporations. Executives set a tone, which may make them culpable and responsible. The Australian criminologist notes that those at the top can create unreasonable pressure that fosters unethical and noncomplying behavior. In a report on his study of the coal-mining industry Braithwaite describes the opposite phenomenon. Safety leaders in the industry had several things in common but the most interesting and generalizable was "a corporate philosophy of commitment to safety and communication of the message that top management did not perceive cutting corners on safety to achieve production goals as in the interest of the corporation." He concludes that "if we wish to maximize the compliance impact of additional investment in enforcement, the most productive targets will be chief executives."

A particularly strong chapter is Byrne and Hoffman's which uncovers the religion that sometimes masquerades as scientifically tight analysis of organizational behavior. This is a careful and comprehensive critique of the law and economics school that has dominated studies of organizations in recent years. Special attention is given to antitrust, but the chapter has broader implications and employs a wide literature.

The breadth and coverage of the volume are appropriate even if difficult to circumscribe conceptually. There is, for example, a chapter on factors that influence the exercise of prosecutorial discretion, written by a former U.S. attorney. This is a lawyer's article but it is written clearly and may be of general interest to those who wish to explore within the black box of the enforcement variable. Rakoff educates us well on constitution-based limitations, as well as on ethical and eye-opening practical considerations in the exercise of discretion, including the prosecutor's estimate of the propriety of the motives of the complainant.

Fisse and French have assembled some very good thinking by a leading group. The book could have been somewhat more fully edited (the same few cases and anecdotes are used throughout); nonetheless it should be welcomed by all students of corporate compliance. It moves the interdisciplinary understanding of the corporation's reaction to law a large step forward.

JOSEPH DIMENTO

University of California, Irvine

Controlling Bureaucracies: Dilemmas in Democratic Governance. By Judith E. Gruber (Berkeley: University of California Press, 1987. xi, 232p. $29.95).

Try ordering a bureaucrat around, Judith Gruber argues, and the result is likely to be trouble. The effectiveness of an agency's functions will suffer, and enforcement of controls can consume vast resources. Understand how the agency goes about its work and how the bureaucrat views his or her job, on the other hand, and things can be different, for bureaucrats see some controls on their behavior as acceptable, or even useful, and will accept them in exchange for the important resources of outsiders.

Gruber's focus is the classic question of how unelected officials can be democratically controlled. Her book begins with an analysis of the options for controlling bureaucracies. It continues by linking various forms of control to theories of democracy, and by specifying their costs. Then comes evidence, gathered from interviews with housing, school, and fire-department employees in a "moderate-sized northeastern city," on the ways bureaucrats view themselves and their work. A theoretical chapter follows, on the differing meanings and forms of control in differing policy areas; and then the interviews are reintroduced in order to show how bureaucrats perceive such control efforts within their own domains. A final chapter returns to basic questions and begins to develop the notion of "control as exchange."

This is a successful book. It pulls together, in a disciplined way, much of what we know about bureaucracies and the ways we have tried to control them. And instead of relying for evidence upon hypothetical cases or anecdotes from policy battles long gone by, Gruber uses her interviews to construct a much more dynamic picture of the bureaucrat's world than we are typically given. The result is an understanding of the generally dismal record of bureaucratic-control efforts, and a surprisingly hopeful conclusion that we can indeed do better.

"Doing better," however, will require a sophisticated understanding of the bureaucrat's world, and of the variations to be found in different policy areas. This book offers the beginning of such an understanding, but much remains to be done. The notion of control through exchange is presented and briefly discussed at the end of the book. I wished for a full chapter offering a more complete development of the idea, perhaps showing how it might be applied to the three agencies under study. Another issue underlying the whole analysis—that of whether or not the people and or their elected representatives are capable of democratic self-government—also needs more discussion than it gets. And while an organization's environment is identified—along with its core technology—as a factor defining important variations among policy fields, the idea seems quickly to lose out to technology as a focus.

But these complaints amount mostly to a wish to see the analysis extended further. The book is well argued and is rigorous at the theoretical level while remaining sensitive to the variations to be found among public agencies and types of policy. By talking to the bureaucrats, listening with a sensitive ear, and carefully presenting their views, Gruber has avoided the aridity which characterizes too many discussions of bureaucracy. And her conclusions on control-as-exchange remind us that, while our elected representatives often turn over significant funds and powers to unelected officials in the pursuit of goals they have only vaguely defined, bureaucrats still need the resources and backing of outsiders. Within their agencies, everyday concerns may well be defined in administrative terms, not in the language and concepts of democratic theory; but politics still matters, and therein lie opportunities to reconcile bureaucracy with democratic control.

MICHAEL JOHNSTON

Colgate University

Going Public: New Strategies of Presidential Leadership. By Samuel Kernell (Washington: Congressional Quarterly, 1986. xi, 251p. $16.95, cloth; $11.95, paper).

Over the past few years, the number of studies examining the presidential leadership strategy termed *going public* has mushroomed. Undoubtedly spurred in large part by Ronald Reagan's frequent use of the strategy, these studies grapple with what constitutes *going public*, why it is on the rise, and how to determine if it is successful. Samuel Kernell's book, *Going Public: New Strategies of Presidential Leadership*, is the latest of these works and is certainly among the best. Still, Kernell's effort like those before, leaves the inquisitive presidential scholar with many lingering questions.

The book defines *going public* as a president's promoting "himself and his policies in Washington by appealing to the American public for support" (p. 1). Such appeals are on the increase, according to Kernell, because institutionalized pluralism is being replaced by individualized pluralism; the latter characterized by individuals who have few group loyalties and prefer immediate payoffs. In short, as the modus operandi of the Washington political community has shifted, Neustadt's bargaining president has been replaced by a public president. One is left wondering, though, how completely going public has actually supplanted bargaining. After all, an administration pursues hundreds of policy initiatives during its tenure, far more than its president could publicly push.

More troubling, however, is Kernell's reliance on individual pluralism as an explanation for the rise in going public. A more complete job needs to be done of disentangling the complex relationship between the shift from institutionalized to individualized pluralism and recent advances in communications and transportation. Kernell correctly recognizes that going public would be virtually impossible without telecommunications and high-speed air travel, but he is less clear about where these technologies fit in the causal chain culminating in the public presidency. It seems quite plausible that a president with access to modern technology would often opt to go public even if bargaining reigned supreme in Washington. Put differently, the relationship between individualized pluralism and going public may prove spurious when controlling for advances in technology.

Also intertwined among these forces is the Washington press bureau. Kernell does a superb job of summarizing the changing relationship between the president and his press corps, but his treatment of the role of the press in the dynamic process of going public is less complete. Does the aggressive contemporary Washington bureau force a president to go public, perhaps even "create" a public president? Or does going public shape the press, perhaps making it part of the new individualized pluralism? The reciprocal relationships between these and other key forces need to be fleshed out, first in theory and then in empirical analysis.

The book excels as a chronology of going public. A good number of examples are described in detail, such as the Truman Doctrine speech and Reagan's budget crusades. *Going Public* would be strengthened, though, if a comprehensive scorecard were kept. How often have presidents' public appeals met with success? Also useful would be a more complete breakdown of going public by administration, party, policies (e.g. domestic or foreign), medium used, and whether the appeal was national or group-specific.

The strongest section reexamines an old question, What determines presidential popularity? Kernell argues that public approval is the "keystone" of going public; that without it neither the populace nor the Washington elite will likely be persuaded by presidential appeals. Maintaining a high popularity rating is crucial, then, to the success of the new leadership strategy. And through some nifty original analysis, the author demonstrates conclusively that high popularity and healthy economic conditions go hand in hand.

Going Public addresses many of the tough questions raised by the emergence of the public presidency. While its answers sometimes fall short, the book successfully frames these questions, thus paving the way for much fruitful research in the future. For this, Kernell deserves substantial praise: bringing some order to such a complex phenomenon as going public is no easy task.

TOM RICE

University of Vermont

Taking Reform Seriously: Perspectives on Public Interest Liberalism. By Michael W. McCann (Ithaca, NY: Cornell University Press, 1986. 345p. $29.95).

It has been roughly 20 years since the liberal public-interest movement began to gain widespread attention for its advocacy in Washington. During this time there has been little critical evaluation of the movement's goals and political ideology. This is the task that Michael

McCann sets for himself in his new book, *Taking Reform Seriously.*

In the first half, McCann offers a careful and detailed analysis of the political and social philosophy of the liberal public-interest movement. This was no easy task, as there is no collection of great works by public-interest activists to consult nor any commonly accepted manifestos to review. To describe the world view of the public-interest movement, he had to piece together the work of a wide variety of scholars and activists from a highly diverse set of sources.

McCann's synthesis of the political ideology of the liberal groups alone makes this book an important contribution to the literature. But he goes much farther to provide a thoughtful and provocative argument that the philosophy of the movement has in many ways worked to undermine its long-term viability. He emphasizes how the movement's consumer and environmental proposals ignore the very real concerns of working-class citizens. McCann views the liberal groups as the political arm of an elitist class, far removed from the mainstream of U.S. life and thus all the more vulnerable to the vicissitudes of the political environment.

McCann's harshest criticism is directed at the movement's failure to help build a true participatory democracy. He notes that "the reformers have managed to mobilize voluntary support from millions of card-carrying members," but at the same time "they have actually engaged very few of these members as genuinely active participants in political life" (p. 170). Although it is one of their pronounced goals, Washington-based public-interest lobbies have generally done little to nurture and revitalize citizenship in the United States.

McCann sees the tensions and contradictions of public-interest liberalism as a cause of its downfall. After many important victories in its early years, the movement is said to have faltered badly as opposition to its goals has mounted. In this vein, McCann makes a surprising judgment about the performance of public-interest groups during the Carter years. He holds that the movement's serious decline began at this point as public-interest groups found the Carter administration to be "a bitter disappointment" (p. 127). There were some disappointments to be sure, such as the defeat of the proposed Consumer Protection Agency. Yet day in, day out, public-interest lobbyists had generous access to agency policymakers throughout the administration, and they played an important role in developing public policy during those years. At the USDA Food and Nutrition Service, for example, former public-interest lobbyists ran the whole shop. The open access that hunger lobbyists enjoyed at this agency would make a Tommy Boggs or Charls Walker jealous.

McCann's view of the Carter years is important because it goes to the heart of his argument that the movement has suffered a fundamental decline because of its unsound philosophical foundation rather than a more transitory downturn because of a turn to the right that came with the election of Ronald Reagan. There's room for disagreement on this question, but McCann weakens his own case by not offering clear criteria for evaluating the relative level of success for these groups.

Although I wasn't convinced on all counts, I found *Taking Reform Seriously* to be an original and enlightening book. McCann's work is valuable reading for all those interested in contemporary interest-group politics.

JEFFREY M. BERRY

Tufts University

Thinking in Time: The Uses of History for Decision Makers. By Richard E. Neustadt and Ernest R. May (New York: Free Press, 1986. 329p. $19.95).

Few would quarrel with Machiavelli's observation that in order to know what is going to happen, one must know what has happened. But the lessons of history can be obscuring and misleading as well as illuminating and orienting. Clio is an enigmatic muse, and her analogies are Delphic. Today, we would hardly share young Ranke's optimism when, in a fervent address to Goethe in 1817, he spoke of "the solid ground of history."

This collaborative work by Neustadt and May, which is based in large part on their jointly taught course at Harvard on the pitfalls of decision making, returns to the major theme of a previous book by May on the (misapplied) "lessons" of history (*"Lessons" of the Past* [Oxford University Press, 1973]). The present study is replete with examples and case studies, mostly drawn from the U.S. experience, that illustrate the dangers of being guided by historical analogies that turn out to be inappropriate for the contemporary context to which they are applied. The central theme of the book is that policymakers often use history badly and that they must learn to use it more discriminatingly. Neustadt and May recommend that decision makers divide the elements of a decision situation into three categories—*known, unclear,* and *presumed*—and that they carefully examine historical analogies to determine if and how they are applicable to the case at hand. Issues should be identified clearly, their "time lines" should be connected with considerations of feasibility, and events should generally be seen within the "stream of time." The authors argue that even marginal improvements in decision making are worth seeking, and that they are obtainable if one would only raise the helpful journalistic questions *who, what, when, why* and *how.*

The "catalog of issues" developed by Neustadt and May is drawn from both domestic- and foreign-policy issues, it includes both successes and failures, and it is extensive. The case studies are examined in some detail and are grouped under the following headings: "Drawing Analogies" (Vietnam, the onset of the Cold War, Korea, the 1976 swine-flu scare, Verdun, and Versailles); "Inspecting Issue History" (the Mayaguez incident, the Pittsburgh political machine, Social Security, and wage and price controls); "Probing Presumptions" (the Intelligence Process, the Bay of Pigs, and U.S. aims in the Korean war); "Placing People" (Mary Anderson and the Women's Bureau, J. W. Fulbright, Martin Luther King, Malcolm X, Frances Perkins, and Vietnam advisers); "Placing Organizations" (the CIA and the United Mine Workers of America); "Noticing Patterns" (economic growth, Keynes and Friedman, U.S. analyses of the Soviet economy, and other cases); and "Judging Change" (the Great Crash, the Civil War secession, Prohibition, George Marshall, and George Washington).

This book is full of common sense and the wisdom that its persistent application engenders. It is a remarkable achievement, rich in analytical insights, written in a lucid and relaxed style, measured and fair in its political judgments. And yet there is something curiously unsatisfying about this enterprise, perhaps because its central premise does not fully correspond to what we experience in our daily lives. Surely we (as well as decision makers) make our choices not only by considering possibly appropriate analogies that come to us from the past, but also by interpreting those that are offered to us by the present. We apply the lessons we obtain currently in one context to the decisions we need to make in another context. We may see ourselves in a diachronic stream of time, but we also occupy a place in a synchronic range of space. Our use of "history" is not only linear and vertical but lateral and horizontal. Indeed, there is ample evidence that decision makers are guided in their actions by present as well as past experience—and both may be misapplied. For example, our early involvement in Vietnam in the fifties stemmed both from misplaced historical analogies of the past and from the seductive contemporary "lesson" that dividing Vietnam could, like the concurrent cases of Germany and Korea, serve as the basis for an accommodation with our opponents. We sought to apply the principles and assumptions of our containment policies, which were taking hold in Europe, to other parts of the world, where they obtained little leverage.

By placing such a heavy and exclusive emphasis on the dangers of improper thinking in *time,* Neustadt and May ignore the equally compelling and perilous analogies that come from thinking in *space.*

WOLFRAM F. HANRIEDER

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Liberalism at Work: The Rise and Fall of OSHA. By Charles Noble (Philadelphia: Temple University Press, 1986. x, 292p. $29.95).

Liberal reforms such as the worker protection regulation implemented by the Occupational Safety and Health Administration (OSHA) are unlikely to succeed in the United States without strong grass-roots organization of beneficiaries—in this case workers—and greater willingness on the part of the state to intervene more directly into the investment decisions of the corporate United States. Noble's underlying argument in this book is not simply that OSHA failed to have much impact on worker safety and health but that reformers missed a major opportunity to use this legislative initiative in order to introduce structural changes to alter power relationships at the plant level, where dominant corporate interests work against safety and health improvements.

During the past century of capitalist development, market pressures have led management to two strategies that ultimately lead to underinvestment in worker safety and health, one emphasizing full control over the production process and the other emphasizing ways to minimize costs of production. Factors such as wage premiums, production losses attributable to accidents, and the need to conserve labor may partially offset investment disincentives, but Noble notes the problems with each of these factors and concludes that they are not enough. Liberal statist reforms developed to counter recurrent problems of workplace safety, primarily workers-compensation schemes and factory legislation, are also ineffective because weak government agencies were never given sufficient authority and enforcement resources to challenge the dominance of business interests seriously, particularly in matters affecting the production process. Only workers organized at the factory level could have provided the government agency with sufficient resources for effective intervention in production processes, but the one likely source of this organized effort, the industrial unions, had chosen to seek economic rewards rather than challenge management's strategy of controlling the production process. In combination, reforms favored by unions and liberal reformers, according to Noble, are doomed to failure unless the need for structural changes are recognized.

Compared with the half dozen existing political-science studies about OSHA, Noble's broad historical account of capital-labor-state relations in the United States as they impinge on safety and health issues in the workplace uses more radical terms to analyze government interventions in "industrial capitalist economies." Yet the tale Noble tells provides a skilled analysis of familiar themes of regulatory capture. Increasing workplace problems during the 1960s discredited existing health and safety policies and indicated rank-and-file discontent. A number of fortuitous circumstances led to the passage of OSHA, including analytic groundwork by a few legislative activists, the political entrepreneurship of the Johnson White House intent on appealing to union workers, the support among Democrats in Congress for protective social legislation, and the disarray of business lobbies. Noble's detailed account of the origins of OSHA, based on interviews and memos from administrative officials, is one of the most interesting contributions of the book to understanding why a major legislative bill emerged despite the disinterest of unions (the presumed beneficiary). Factors increasing the impracticabilities of policy processes are well portrayed, particularly in the amusing account of how business allies inadvertently strengthened the bill with suggestions intended to weaken it.

The rest of the story is familiar but not as convincingly told. Business groups organized to oppose the burgeoning social regulations, while labor failed to take initiative. With the help of a declining economy and a White House review process responsive to economic concerns of business and academic allies, businesses succeeded in rehabilitating the values of market capitalism. OSHA's activities were systematically curtailed. "By the end of 1984, social regulation was dead in the water" (p. 161). But the accounts of changes in OSHA's actions, particularly under Carter, point out the variety of forces behind these changes, many of which counter the main theme of inevitable decline. OSHA is found to have some effect but not enough. Noble does an excellent job of documenting the political forces aligned against "liberal" reforms based on regulatory agencies but says little about how his preferred strategy of increasing worker controls at the factory level and more extensive state controls over investments is likely to bring about greater effects in the same political setting. Similar strategies to mobilize new groups in the war on poverty politically, failed for predictable reasons. Nonetheless, Noble's broad perspective, extensive scholarship, clear presentation, and reformist sympathies make this book a delightful change from the normal market perspective currently dominating discussions of social regulation.

JOHN SCHOLZ

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Book Reviews: American Politics

Alternative Americas: A Reading of Antebellum American Culture. By Anne Norton (Chicago: University of Chicago Press, 1986. ix, 363p. $24.95).

This book follows the tradition of Tocqueville, Louis Hartz, Sacvan Bercovitch, and others who have attempted to explain the United States' cultural tradition. In *Alternative Americas*, I think we have a valuable, although perhaps controversial, addition to that corpus. Although her narrative encompasses antebellum culture in the U.S. West as well as in the North and South, her primary focus is on the North and South, and her major contribution lies there.

Norton argues that U.S. political culture prior to the Civil War was a political culture marked by an increasing separation between the North and the South. This separation reflected a difference between the regions that arose not because of a disagreement over slavery, or between different views of federalism, or even between the existence of an increasingly commercial North and a feudal South. These arguments have been made by others. Norton argues that such views of the South "ignore the language of the era" (p. 4). Instead, she argues that the distinction between the two regions is one that is fundamentally and explicitly sexual in nature: "Throughout America, throughout the antebellum period, the North was identified as masculine and the South as feminine, in an elaborate constellation of metaphors" (p. 4). These perceptions of the South and the North are revealed in the relationship that developed between white Anglo-Saxon Protestant males and groups which were marginal, or liminal, to these societies. In the North, the liminal groups were immigrants—especially the Irish—and women. Slaves, women, and Indians were the liminal groups in the South. In the case of both regions, however, the relationship between liminal and dominant group was perceived in sexual terms. Thus, Norton argues that in the North, the needs of industrialization resulted in a valuation of "masculine traits," for instance, rationality, hierarchy, and community defined by law. The South, with its primarily agrarian economy, tended to see itself primarily in "feminine" terms, valuing self-expressiveness, individual autonomy, and emotionality.

The different development of North and South has important consequences for the relationships between the dominant and liminal groups in each region. In the North, feminine characteristics were ascribed to liminal groups, and resulted in attempts to "reform" them. The goal here was to assimilate these groups into society not as equals but as part of a docile labor class. Liminal groups were to perform as docile "wives" to a masculine paternalistic industrial elite. Thus, the North proposed a paternal state that would use its power both to control and to mollify. The South chose a different path, appropriating the characteristics of liminal groups and identifying with them. Thus, because they identified with women and their maternity, southerners chose to adopt a model of social relations that mimicked familial ones. For example, Norton argues that southerners identified themselves with the Indian and that that identification led them to describe the supplanting of the Indian by the white man as a fraternal conflict, like that between Cain and Abel. Southerners saw that the Indians "must be torn from the breast to make room for the encroaching whites" (p. 145). The South rejected the model of state paternalism embraced by the North but exhibited a plantation life that was itself paternalistic. In both cases, the relationship between dominant and liminal groups was one that ultimately denied the equality of the members of the liminal groups.

This very short synopsis does great violence to the richness of Norton's argument. However, rich as the argument is, it is not entirely convincing in its discussion of the South. The basis of the South's feminine character is grounded by Norton in the agrarian nature of the region. The continued reliance on the earth, and the resultant ties to natural process of growth and season, she argues, results in the image of the South as "motherland." Two points should be noted here, though. The first is that at bottom, I think it might well be argued that the nature of the relationship between the southerner and the land is not harmonious, but instead domineering. It is plausible that the southerner perceived land as something to exploit, particularly given the historical accounts describing the wastefulness of Southern agricultural methods. This would make their attitude toward nature no more salutary than that of their northern counterparts. Second, although the southerner might well have appropriated "feminine" traits to define his self-image, it seems to me that this led to a redefinition of masculinity—the image of the indulgent, sensual, violent, and independent Southern male is that of the typical "good ole boy," who would quail at the notion that he was "feminine" in any way—rather than a true identification.

Finally, I found it interesting that Norton makes an association between agrarianism, femininity, and a republican political culture. Certainly, Rousseau, that old advocate of republicanism, would find the association of femininity with republican government surprising. One might well ask why this association would take place in the United States, but that goes beyond the scope of this work.

These comments are hardly meant to be criticisms. This book is thought-provoking and well argued and will be of great interest to those who work in political theory, women's studies, Afro-American studies, and U.S. politics. It is a fine book well worth a first taste and, like a fine wine, a second as well.

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Making Cancer Policy. By Mark E. Rushefsky (Albany, NY: State University of New York Press, 1986. xiii, 257p. $39.50, cloth; $12.95, paper).

The sudden proliferation of federal health, safety, and environmental regulatory programs in the 1970s is one of the major events of recent U.S. history and governance, yet one whose substance has received surprisingly little attention from political scientists. For more than 10 years now, a primary objective of these programs has been the control of chemicals that may cause cancer and to that end the elaboration of detailed policies—"cancer risk-assessment guidelines"—setting standards of evidence for use in regulatory proceedings.

Mark Rushefsky's *Making Cancer Policy* provides an excellent analysis of the interpenetration of facts and values, of science and politics, that he argues is inevitable in such a process. Unlike many other books on environmental politics and policy, it is neither an advocacy tract nor merely an account of the interest-group politics surrounding these issues, but a penetrating and insightful synthesis of theory, history, and substantive analysis of a complex and technical policy issue. It should be valuable not only to readers interested in cancer policy, but also to those interested in risk assessment as a form of policy analysis and in the politics of science and regulation more generally.

Four themes run through this material: the problem of imperfect knowledge and scientific uncertainty; the existence of controversy within both the scientific community and the larger society; the inevitability of mixing political judgments and assumptions into ostensibly scientific assessments supporting regulation; and the political uses of science itself by advocates of particular policies. A central principle of recent Environmental Protection Agency (EPA) policy, for instance, is that "risk assessment" is a purely scientific enterprise, to be clearly distinguished from "risk management," in which political, economic, and other value considerations are added in and regulatory decisions made. Rushefsky argues persuasively that this distinction is untenable, that policy judgments and value-laden assumptions are inevitably commingled with regulatory uses of science even in the assessments, and that regulations are therefore more appropriately developed through such mechanisms as "consensus dialogues"—providing for open debate over what assumptions are to be used—than through the pretense of scientific neutrality.

Rushefsky develops these themes first by describing the regulatory use of science to produce risk assessments, then by documenting the evolution of cancer regulation and related policies (with particular emphasis on the period since 1970), and finally by critically analyzing the issues that must be addressed in cancer regulatory policy today and the arguments offered by other writers on the subject. For a theoretical framework he uses two sets of concepts: John Kingdon's characterization of policy agenda setting as growing out of the interactive evolution of problems, policies, and political events (*Agendas, Alternatives and Public Policies* [Boston: Little, Brown, 1984]) and to a lesser degree Patrick Hamlet's characterization of technological policymaking as occuring in distinct "arenas" or "estates," each subject to its own distinct incentives, influences, pressures, constraints, and goals ("Understanding Technological Development," in *Science, Technology and Human Values* 9 [Summer, 1984]: 33–46).

Particular strong points of Ruchefsky's book include its mature treatment of all sides in what has become a shrill and contentious policy issue, and its frequent use of concrete cases to illustrate its points. Its title notwithstanding, it does not address all aspects of cancer policy (the politics of cancer research funding, for instance, or the politics of tobacco subsidies); nor does it pursue some questions as far as one might wish. One key question left unanswered, for instance, is whether ideology or science was the primary driving force in the policy changes Rushefsky describes. Were EPA officials (such as John Todhunter, for instance) simply political ideologues who used scientific assumptions selectively to justify their preconceived positions, or were they good scientists articulating changes in scientific understanding whose implications happened to be consonant with the Reagan administration's agenda? Both the reputations of the individuals and our understanding of subject turn on the answers to such questions, yet Rushefsky stops short of answering them.

Notwithstanding these minor criticisms, Rushefsky's book provides a solid and readable scholarly treatment of the regulation of environmental carcinogens and a valuable contribution to the literature.

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Disenchanted Realists: Political Science and the American Crisis, 1884–1984. By Raymond Seideman and Edward J. Harpham (New York: State University of New York, 1985. vii, 295p. $39.50, cloth; $12.95, paper).

The authors of this volume argue two basic theses: First, from the beginnings of U.S. political science there can be traced a lineage of practitioners who constitute a "third tradition." These individuals, all characterized as "liberals," believed that the problems of our U.S. society could be solved by "linking objective professional political study with political reforms." Convinced that a scientific political science could lead to whatever changes were required to make the U.S. democratic system function effectively, they "blended scholarship and political advocacy, a science of politics with a science 'for' politics" (p. 2).

Second, the reformist proposals espoused by the third tradition (from Lester Ward in earlier years to Theodore Lowi and Walter Dean Burnham today) have been not only manifestly but inescapably unsuccessful. Consequently, if U.S. political science is meaningfully to address our current political, social, and economic ills, it must abandon the "centrist liberalism" of the third tradition and move drastically to the right or to the left.

These two propositions, it is important to note, have no inherent logical relationship. That is, the truth or falsity of one has no necessary implications for the truth or falsity of the other—a view, I suspect, with which the authors might not agree. In any event, I will deal with them in order.

According to Seideman and Harpham, there are two long-established traditions in U.S. political thought (a term sometimes employed as synonymous with *political science*)—the *institutionalistic* tradition and the *radical democratic* tradition. The institutionalists (they point to the "American Founders") are skeptical about human political capacities, inclined to put their faith in the Constitution, and utilize "a political vocabulary of system, mechanism, control, realism, skepticism and 'facts' " (p. 5). In contrast, the radical democrats (e.g., Tom Paine) trust popular virtue and sentiment and favor "continuous experimentation and impromptu forms of popular power associated with equalitarian democracy" (p. 6).

The newly discovered third tradition, however, seeks to go "beyond mechanics and spontaneity." It is less a synthesis of the other two, according to Theodore Lowi's Foreword, than "an organized effort virtually to deny that these are separate and inconsistent streams of political thought and values" (p. xiii).

What do they have in common, these eponymous "disenchanted realists" (a phrase that appears so rarely in the text that one senses the publisher's helpful hand) who constitute the third tradition? They are all "liberals" (a term never defined and used with remarkable elasticity); they all believed in a "science of politics" or a "scientific politics" (terms used interchangeably); they regard the state as essentially benign; they do not think that "citizens are by nature greedy, self-seeking and irredeemable"; they have great faith in the possibilities of civic education and, as previously mentioned, of political reform. "Relevance" appears as a specific requirement only in the discussion of postbehavioralism.

The book's opening chapter briefly describes the institutionalistic and radical democratic traditions and then, at somewhat greater length, delineates the beliefs common to the third tradition. Chapter 2 traces the emergence of a professional U.S. political science and the concomitant appearance of the third tradition in the persons of Lester Ward and Woodrow Wilson. Chapters 3–6 carry the story down to the present: chapter 3 focuses on Bentley and Beard, chapter 4 on Merriam and Lasswell, chapter 5 on V. O. Key and David Truman, and chapter 6 on Lowi and Burnham. The treatment is essentially the same in each case: the ideas of each of the aforementioned individuals are analyzed to show how they can be fitted, if occasionally in Procrustean fashion, into the third-tradition mold. What we have here, then, are eight intellectual biographies, running some 10–20 pages in length, interestingly written, and providing a useful, if perhaps sometimes arcane, reading and assessment of eight of our leading practitioners, past and present.

The key question, of course, is whether the authors make a convincing case for their thesis. There are, I must say, some difficulties. As indicated above, key terms are often undefined and loosely used. Not only are the first and second traditions treated in sketchy fashion but, more troublesome, the authors fail to identify a single political scientist associated with either of these traditions, despite their claim that "modern political science has been understood as a simple extension of one or the other of them" (p. 3). This omission makes any meaningful comparison among the three traditions extremely difficult.

That difficulty is further compounded by the professional and political commitments that, the authors contend, uniquely characterize the third tradition—namely, aspirations toward a more scientific politics; a positive attitude toward the state; and faith in human nature, education, and reform. These (and possibly even "relevance") have been the values, I would suggest, of many, if not most, U.S. political scientists rather than of any select "subfield within a minority" (p. 191).

In short, I remain dubious that the eight political scientists singled out in the volume really constitute an intellectually homogeneous set of "disenchanted realists," or that they represent a truly separate third tradition in either the practice of U.S. political science or the history of U.S. political thought. Try as I might, I cannot escape the sense that there is an essentially contrived quality to the thesis. In fact, the authors themselves tell of the slow metamorphosis of a "semester-long reading project among three Cornell graduate students" into "a longish paper, a short monograph, and finally into three or four versions of the present work" (p. xix).

Chapter 7, "Conclusion: The End of the Third Tradition," advances two propositions. The first is that liberalism generally, and especially that variant exemplified by the third tradition, is hopelessly incapable either of correctly diagnosing, or of proposing useful solutions for, the ills of contemporary U.S. democratic capitalism. The liberals cannot do so because, given their inherent ideological limitations, they are unable to comprehend the "political proclivities of American capitalism," the gravity of the problems with which we must deal, or the ineluctable deficiencies of our system of governance.

The second proposition follows from the first. Since liberalism has come to the end of the road, U.S. political scientists *must* move to the right or to the left if they are to have any voice in the reconstruction of U.S. society. If and only if they do so can they hope to play a significant role in the debate between the "advocates of democratic socialism and state conservatism" that "may finally form the contours of future American politics" (p. 240).

Must we finally abjure "centrist liberalism"? Is there nowhere to go but to the statist-conservative Right or to the democratic-socialist Left? I do not know. But I do recall hearing, many years ago, the shattering news that God was dead and, in a probably unrelated communiqué, that we had come to the "end of ideology." Both pronouncements were, I think it fair to say, somewhat premature. So, too, it may be, with the alleged demise of liberalism.

In their Acknowledgment, the authors give special thanks to Theodore Lowi without whose "charm, wit, tolerance, critical abilities and sheer enthusiasm . . . this book would never have been written." As indicated earlier, Professor Lowi has also contributed a Foreword in which, inter alia, he endorses the notion of a third tradition. He contends that "whether by coincidence, common causation or plan, modern social science and modern government have common origins in liberal thought" (p. xxix); and, coming to the immediate point, describes *Disenchanted Realists* as a "splendid and provocative book."

Although *splendid* is not a term that would have occurred to me, I do agree that the book is certainly provocative, both in its strengths and its weaknesses. The entire volume should be read by those concerned with the history of our profession; the final two chapters by those who, if less interested in where we have been, are curious as to where we now are—or might be going.

ALBERT SOMIT

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Citizen Demand-Making in the Urban Context. By Elaine B. Sharp (University: University of Alabama Press, 1986. xii, 211p. $25.95).

Citizen contacts with municipal governments have both intrigued and puzzled political scientists in recent years. These contacts are intriguing as an apparently increasingly common form of political participation, but they are puzzling because they seldom follow the patterns observed on other forms of participation. Socioeconomic status, for example, cannot be counted on to have the same positive effect on contacting propensity that it has on other participation.

Prominent among those who have tried to solve the puzzle is Elaine Sharp of the University of Kansas. Now, as the culmination of a lengthy NSF-supported project, she has produced the best analysis to date of the causes and consequences of these contacts—or *demands*, as she terms them.

This is a carefully crafted, meticulous piece of research. One by one, Sharp analyzes each question about these contacts, translates the question to operational terms, and presents and interprets original data she collected to answer the question. The manner of presentation is almost always clear and well written, thereby making complex arguments understandable.

Sharp is at her best when addressing the causes of these demands. She assesses the role of many possible causes, ranging from the standard variables of socioeconomic status, need, and awareness to the usually overlooked influences of centralized complaint units and neighborhood organizations. In the process, she suggests resolutions to many previously conflicting conclusions about the relative roles of these factors.

Perhaps the most interesting question Sharp attempts to answer is why political science should be concerned with these contacts at all. It could be argued, after all, that contacts focused on mostly minor service-delivery problems are politically trivial. In two concluding chapters, Sharp attempts to show why that is not the case.

She suggests, first, that these contacts can influence whether citizens stay in the city or move away. Evidence presented in the book indicates that dissatisfaction with the municipal response to contacts does significantly, if modestly, increase citizen inclinations to move out of the city.

Sharp is somewhat less persuasive in arguing, second, that these contacts may be important in a negative sense as a component of the supposed "demand overload" on urban governments. This speculation raises a number of interesting empirical questions. Is the increase in these contacts sufficient to constitute a dramatically increased demand? Has the increase translated to an increased demand on municipal resources? These questions are ignored in the book, however, in favor of theoretical speculation founded on a directed literature review. Although interesting, this speculation is less convincing than the book's data-based arguments.

That is a minor problem, however. The only major problem for the book may be finding an audience. Written in the manner of a sequence of journal articles, with each step in the research carefully explained and documented, the book represents a significant research contribution but not a contribution accessible to a broad audience, even within political science.

This is not the sort of book, for example, likely to be used in an undergraduate class, even as a supplementary text for advanced undergraduates. The book is important reading, on the other hand, for any faculty or graduate students who want to understand how citizens interact with local government.

JOHN CLAYTON THOMAS

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Back of the Yards: The Making of a Local Democracy. By Robert A. Slayton (Chicago: University of Chicago Press, 1986. xiv, 277p. $22.00).

This book treats a largely immigrant, early industrial, working-class community that emerged in one of the world's most unpromising settings—the Back of the Yards. In this area, the workers in Chicago's meatpacking industry lived and worked from approximately the end of the Civil War until the 1950s when the industry began to move out of Chicago, culminating in the official closing of the Stockyards in the early 1970s. Meatpacking as a large-scale industry was one of the earliest and largest fully to rationalize the factory system. And within the industry, the Chicago plants were the earliest and largest to do so. By 1900, if not before, Armour and Company could boast, truthfully, that they used every part of the hog except the squeal.

But the minute division of labor that makes such rationalization possible (and profitable) extracted a high toll among those who labored there, particularly so where the labor force was relatively defenseless to begin with, as this one was. From Marx on, sociologists of quite different theoretical persuasions have documented the social pathologies attendant upon early, rapid, large-scale, industrialization. Of all these ills, the one that lies at the crux is social disorganization and anomic behavior, or, in the author's words, "the concept of community lost."

It is this theoretical convergence of both the Marxists and the non-Marxists and their agreement that the loss of community was and is the defining characteristic of a neighborhood like the Back of the Yards that supplies the author with his leitmotiv. He disagrees, and sets out to show "that communities did exist in urban industrial immigrant districts, that bonds between people did form, and that institutions did persevere," "that residents of these inner city areas did form *communities* even by a rustic definition of that term" (emphasis original) (p. 8).

The book is, of course, a case study and the author is conventionally modest about the typicality of the Back of the Yards and consequently about the generalizability of his findings. He is too modest. The point to be asked about any empirical study is not, primarily, how the evidence was generated but rather whether the findings are theoretically interesting and relevant. At this level, there is no essential diference between a case study and any other mode of inquiry. And there can be little doubt that this study satisfies on that score. There are other advantages to the choice of this site as well. Many of the changes in which the author was interested have occurred within living memory and, accordingly, the text is enriched by a number of lengthy interviews (79, by my count). Because of the location and the time, there is a wealth of publicly available data that studies of early industrialization in other parts of the world have often had to do without. Finally, the Back of the Yards has been the object of scholarly scrutiny for a long time; most notably by the social scientists of the nearby (geographically if not socially) University of Chicago, who, as early as the turn of the century, were already treating the neighborhood as their own private laboratory.

The author makes his case that a community—or rather communities—in the full sociological sense of the concept of *community*, did emerge in these unlikely environs—and he makes it persuasively. Very early on, the immigrant workers recreated as best they could the social conditions and institutions they had left behind in Europe (the book concentrates on the middle period from roughly 1880 to 1920 when the immigration was overwhelmingly East European). Primarily these were based on church and family and therefore faithfully recreated as well the ethnic balkanization of Eastern Europe. Shared language and belief and very often even shared villages or districts of origin in the homeland facilitated the development of a sense of community in a strange and hostile environment. In addition, a network of other forms of association such as sport clubs or social clubs soon sprang into being. The churches were fertile sources of such activity. The author at one point counted 18 different associations with no direct religious purpose sponsored by one church. Neighborhood taverns and shops provided daily foci for socializing and socialization. In short, a rich mosaic of associational life that provided the social and psychological support for the people to assert their autonomy and establish control over crucial aspects of their lives soon emerged. Their struggles were not, of course, uniformly successful. In particular, the attempt to establish some degree of autonomy in a central aspect—the workplace—through unionization was, on balance, a failure until the late 1930s, in part because the packers manipulated the very ethnic divisions that in other ways served to facilitate the development of community.

There is, however, a parallel thesis to the one concerning the establishment of community. That is that the creation of community simultaneously involved the creation of democracy. Indeed the book is subtitled, *The Making of a Local Democracy.* And here caution is advised. For one thing, at the conceptual level community and democracy are by no means the same thing. Some—Burke, for example—have even seen them as antithetical. Yet the author at times confounds the two. We can agree with the author that the Back of the Yard-ers sought, and in no small measure achieved, stability, predictability, and autonomy in the sense of freedom from external interference in many areas of their daily lives. But nomic, as opposed to anomic, behavior or autonomy, as opposed to dependency, can be realized quite apart from democracy. It must be shown, not assumed, that the creation of community entails the creation of democracy as well.

That demonstration is particularly necessary here. The two major institutions responsible for the development of community—church and family—are late nineteenth century, East European, and working-class. Rightly or wrongly, such structures have most often been characterized as strongly authoritarian and fundamentally antidemocratic. Second, two of the major external actors the author cites as helping to foster democracy, the unions and, somewhat later, the local Democratic party machine were not and are not internally democratic. The author says that the union, for example, extended democracy, "by organizing industrially, by company, rather than by craft." But how, one asks, does this extend local democracy? And the local political machine may have imbued the residents with the sense that "downtown" can be manipulated, but clearly it did so in the framework of a patron-client relationship rather than democracy.

Finally, one must remember that all of this takes place within a larger picture. And that larger picture is the overwhelming dependence of the community on the meatpacking industry with all the misery and degradation that entailed together with the overall failure of the workers to establish order and autonomy in this central aspect of life; failure, that is, until 1939, nearly 80 years after the community was founded and less than 20 years before the industry started to disappear from Chicago. Summing up, the book more than amply shows the emergence of a community in a setting where one would think it unlikely. It does not successfully demonstrate the emergence of local democracy in an equally unlikely setting, in part because the author assumes what needs to be shown—that community and democracy go together.

DON R. BOWEN

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Public Opinion and Collective Action: The Boston School Desegregation Conflict. By D. Garth Taylor (Chicago: University of Chicago Press, 1986. xiii, 241. $29.00).

Reading this book brought back a lot of memories, many of them unpleasant: violence in the streets and schools of Boston; tawdry attempts by Louise Day Hicks, "Pixie" Paladino and John Kerrigan to exploit the desegregation controversy to advance their political agendas and careers; timid leadership by Mayor Kevin White, concerned about his own political skin; ineffective leadership by Cardinal Medeiros as he attempted to rally his flock, in this most Catholic of the United States' great cities, behind peaceful support of desegregation. Garth Taylor, I should hasten to add, does not attempt to exploit the sensational aspects of the Boston school-desegregation controversy. This book is a careful and skillful piece of work, written in the straightforward style suitable for reporting the results of social-science research. For anyone interested in how a city, once the cradle of liberty, nearly became its coffin, this book is most instructive.

Taylor begins with a review of racial ideology and civil-rights progress in the United States. By the time of *Brown v. Board of Education,* he concludes, U.S. citizens were prepared to jettison racially based doctrines of inferiority, social inequality, and mandatory segregation. Over the next 10 years, Taylor argues, there was widespread acceptance of the right of black students to attend desegregated public schools. In this process, the focus of debate shifted from whether mandatory segregation was wrong to whether mandatory desegregation was right. People were prepared to abandon the former but not necessarily to accept the latter. Orientations toward racial injustice came to be governed by what Taylor calls the "Doctrine of Voluntary Compliance," a belief that the wrongs of mandatory segregation are remedied best by voluntary desegregation. Taylor traces the roots of this doctrine back to Reconstruction and demonstrates how it can still operate as a powerful deterrent to mandatory desegregation.

The analysis is based mainly on five waves of survey data from a panel of 631 white adult residents of six neighborhoods in the city of Boston. The interviews were conducted over a period of approximately two years, beginning several months before Judge Arthur Garrity's order to desegregate the schools, and ending after the implementation of the first phase of the plan.

Taylor shows that opposition to busing and willingness to protest against mandatory desegregation are not merely the product of racial prejudice and intolerance. Adherence to the doctrine of voluntary compliance provided a frame of reference for citizens to view the insistence on mandatory desegregation as contrary to U.S. principles of popular consent and thus as unfair and unjust and potentially harmful in terms of safety and educational quality. Equipped with these "rationalizations" rooted in a pervasive popular belief, citizens became prime targets for collective mobilization at the hands of antibusing leaders whose own political prosperity was linked to the stridency and duration of public protest. In telling the story of how a city with liberal and abolitionist traditions came to symbolize bitter and protracted opposition to school desegregation, Taylor shows how all the pieces of the puzzle came together: widespread belief in the unfairness and potential harm of the desegregation orders; aggressive antibusing leadership; a strong and resourceful organizational base for protest and a belief in its efficacy; limited resources for support, enfeebling the initial efforts of those charged with enforcing compliance with Judge Garrity's orders.

Although published in 1986, this book, based on research conducted in the 1970s, belongs very much to that decade. Like much research of that time period, its main focus is on white resistance to desegregation, and Taylor does an able job of relating his work to the research of that period. Perhaps in recognition of the somewhat dated quality of the work, Taylor attempts to draw some contemporary significance from the Boston case. In the final few pages of the book he notes that the federal-court desegregation orders produced significant changes for the better in the Boston public schools, improving management and curriculum, raising average daily attendance, and boosting dramatically the proportion of high-school graduates going on to postsecondary education. A few years after the disruptions that accompanied the first phase of implementation, the situation changed dramatically. Hicks, Paladino, and Kerrigan all suffered electoral defeat, the Boston school board came under the control of blacks and moderate whites, and, in 1985, the federal district court closed the case. While Taylor (reminiscent of the poet John Donne) seems to feel that the old order of mandatory segregation is dead but that the new order of a desegregated society is yet powerless to be born, an opposite conclusion can be drawn. A new order of school desegregation has indeed been born in Boston, and its benefits are being reaped by those who chose not to flee in protest but to remain in voluntary compliance.

EVERETT F. CATALDO

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Book Reviews: American Politics

Reagan's America: Innocents at Home. By Garry Wills (Garden City, NY: Doubleday, 1987. 472p. $19.95).

Most political scientists who study U.S. politics and virtually all who study the presidency are likely to have read at least parts of one or more of the previous books in which Garry Wills combines history, biography, literary imagination, facile prose and self-assured personal judgements in accounts of such U.S. presidents as Washington (*Cincinnatus* [Garden City, NY: Doubleday, 1984]), Jefferson (*Inventing America* [London: Athlone, 1980]), Madison (*Explaining America* [Garden City, NY: Doubleday, 1978]), Kennedy (*The Kennedy Imprisonment* [Boston: Little, Brown, 1982]) and Nixon (*Nixon Agonistes* [New York: New American Library, 1979]).

Wills's latest foray is only partly about the current chief executive. Rather, as the possessive in the title indicates, it also comments on aspects of U.S. politics, society, and history that nurtured Ronald Reagan—for example, the small-town Midwest, the ethos of the Disciples of Christ (Reagan's mother's religious denomination), life in the Depression, and the myths and mores of Hollywood films and the film colony. But the book also delves extensively into Reagan and his career, refracting both Reagan and his America through the imagery of Mark Twain (hence the variant on *Innocents Abroad* in Wills's subtitle).

Any reader of this book who has *not* read Wills's previous work and expects to encounter meat-and-potatoes journalism of the sort to be found, for example, in Lou Cannon's *Reagan,* will be disabused before finishing the first paragraph of *Reagan's America.* Wills pronounces that Reagan must be understood as a *synecdoche* (a figure of speech in which a whole is described by naming an instance of it, e.g., "He is a Rockefeller" for "He is a millionaire"). Indeed, the political scientist who is unfamiliar with the Willsian mode is likely to dismiss the book as hopeless before completing that paragraph with its succession of fuzzy assertions of paradox: "The geriatric 'juvenile lead,' even as President, Ronald Reagan is old and young—an actor, but with only one role. Because he acts himself, we know he is authentic. A professional, he is always the amateur. He is the great American synecdoche, not only a part of our past but a part of our multiple pasts. That is what makes so many of the questions asked about him pointless. Is he bright, shallow, complex, simple, instinctively shrewd, plain dumb? He is all of these things and more" (p. 1).

Why then should a reviewer ignore the book-review editor's instruction not to submit a review "if, after examining the book, you conclude it is not appropriate for *APSR* treatment"? For this reader, the reason is that by the end of this sometimes maddeningly digressive book (and in spite of its reiteration of the familiar theme that Reagan is a font of rhetoric, with little if any impulse to confront his rhetoric with evidence), I had a visceral sense of the character and roots of the fortieth president that I heretofore lacked—a sense of Reagan as a man, a sense not easily specified but that goes beyond what I learned from Cannon's scrupulously detailed biography.

Some of this sense has a cognitive basis. Wills has unearthed new information about Reagan's hometown. He interviewed people who knew Reagan in his early years, mined the accounts of him in 1940s film magazines and Hollywood gossip columns, and reassessed his participation in the Screen Actor's Guild and the biographical record of this film-actor-turned-politician.

Much of what led me to continue reading, however, was the book's imagery and its author's undocumentable interpretive flights. In the mode of a novelist or essayist, Wills stretches the reader's imagination. Although he would deride such social science usages as "heuristic," the fertility of Wills's conceits, along with the archness of his prose, provides this distinctly non political science–like analyst with his political-science following.

Moreover, some readers may have reason to believe that even when Wills's assertions are unsupported, they may be on the mark. Long ago I read and dismissed as clever fantasy Wills's account in *Nixon Agonistes* of a tough-minded, politically shrewd Dwight Eisenhower. Later I found myself opening the newly declassified personal papers in archives of the Eisenhower Library. What Wills put forward as unquestioned truth on the basis of no evidence was on the mark and he had been spared my own molelike burrowings through mountains of documents.

This book is vintage Wills. It is not as carefully assembled and closely presented as some of his earlier work. It is hard to say much new about Ronald Reagan at this point, and Wills's two purposes of discussing Reagan and discussing Reagan's America get tangled with one another. Still it is a safe prediction that just as many political scientists have found Wills's earlier books of interest, many will read or dip into this one. They will not find political science but are likely to emerge with a more nuanced perspective on a president who needs to be deciphered in any effort to take stock of the continuities and variations in presidential leadership.

FRED I. GREENSTEIN

Princeton University

COMPARATIVE POLITICS

Capitalism and Public Policy in the UK. By Tom Burden and Mike Campbell (London: Croom Helm, 1985. vii, 269p. $29.00).

In the authors' own words, this book "aims to provide an analysis of the nature, development and impact of public policy in the UK" (p. vii), and it attempts to do so from within an explicitly Marxist framework. Ambitiously, nine topics are covered, ranging from the more or less expected economic policy and "health and welfare" through the less studied "cultural policy" and "law and order," concluding with a look at "foreign and defence policy." Each of these chapters is clearly organized, beginning with a section outlining the relevant theoretical concerns, after which the course of British public policy in that area is outlined. *Policy* is generally taken to mean "legislation," with special reference to the post-World War II period. Finally, the chapters conclude with a brief section that attempts to link the pattern of acts back to the Marxian model. In very general terms, the argument presented is a familiar one: the state pursues policies (passes laws) that are in the interests of the capitalist class, and particularly the financial (city) interests of that class. The succession of acts, then, can be understood as a means by which such processes as "accumulation" and "legitimation" are made more secure for capital.

Despite the fact that there is little (if anything) that is new in this argument, such an approach does come as an antidote to the pluralist conceptions of the state, which often view the state as a neutral arbiter of interest-group demands. At the very least, a Marxist approach gives pause for thought to the pluralist "state-as-referee" idea.

Valuable though this insight may be, however, its worth hinges on an ability to demonstrate evidence for the argument, a point especially critical when there are important problems of falsifiability, as is often the case with Marxist approaches to public policy. Unfortunately, Burden and Campbell, while able to present a redescription of many pieces of legislation in terms of their capitalist interest, do not tackle, but only court, these problems. The "capitalist interest" seems to cover many eventualities and aspects, and can be defined and redefined in such ways as to ensure that every government act, without exception, was in the interests of capital in at least one sense: if not accumulation, then legitimation; if not in the short term, then the long term; if not for capital as a whole, then for some "fractions" of capital. The latter device does open up some interesting room for discussion. After all, why should successive British governments be so slavishly devoted to financial interests at the expense of (job- and vote-providing) business interests? Rather than being a starting point for analysis, however, such questions are an end point: they are not pursued and are rarely in any form amenable to hypothesis testing.

At some points the authors do present opinions in what may be termed *testable* form. For example, "The worse the condition for profitable accumulation, the greater the 'needs' of capital, and the more coherent their articulation, then the less the strength of labour and the less is the need to buy consent" (p. 73).

Sadly, these all too rare moments do not result in any attempts to test out the hypotheses. Here the book's content does not follow its systematic form, and in general there is no direct demonstration of the links between policy outcomes and capital, only the repeated assertion that such and such a policy is consistent with some flexible definition of the capitalist interest. More detailed and careful studies of the relationship between business and policy, such as those conducted by Moran, Grant, or Marsh tend to reveal that the situation is usually much more complicated and uncertain. Often there is little evidence to support the view that the strings of politicians are being directly pulled by businessmen.

All in all, readers will find very little here of immediate use in studying British public policy; more praiseworthy and useful studies do exist in which at least some of the same conclusions may be reached with a much more parsimonious theoretical underpinning. Having said that, however, the emphasis on the importance of capitalist interests and of a state structure that is not politically anemic are important points. At the very least, they demand some kind of response from those students of policy who rely on such diverse analytical foundations as electoral incentives, the importance of trade unions or the impact of national styles. Despite the fact that many political scientists would agree with Lindblom's assertion that there is a "privileged position" for business, all too few seem to wish to see how this works out in practice and in relation to other, competing, explanations.

SHAUN BOWLER

Washington University

National Politics and Community in Canada. Edited by R. Kenneth Carty and W. Peter Ward (Vancouver: University of British Columbia Press, 1986. 200p. $14.00).

Six historians and four political scientists combine in this collection of essays to argue for a reinterpretation of the evolution of Canadian politics and the Canadian political community—or, perhaps more precisely, to call for a return to an older tradition. Two themes dominate. The first is substantive. Canadian scholars, the editors argue, have been too preoccupied with "the part and not the whole" (p. 1)—with the weakness of the national political community and its institutions, with regional and linguistic cleavages, and with "province building" and the decentralized character of Canadian federalism.

Such preoccupations, they argue, underestimate the "primordial" strength of the national political community and the role of institutions in fashioning and maintaining it out of "the pieces of Britain's North American Empire" (p. 1).

The methodological theme, therefore, is "state centered," emphasizing the role of political parties and their leaders in using the resources of the state, primarily those of patronage, to build networks of interest and loyalty: "The national community was explicitly political in early Canada, where party constituted the principal tool of pragmatic nation-builders" (p. 7).

This nation- and state-centered approach is indeed an important corrective to recent trends of interpretation in Canada. Several of the authors elaborate it persuasively—Gordon Stewart with an excellent analysis of the foundations of elite culture and style in pre-Confederation Canada; the editors in their analysis of the changing definitions of citizenship through manipulation of electoral laws; P. B. Waite in a lively description of the role of spoils and jobs in reconciling the reluctant entrants Nova Scotia and New Brunswick to Confederation; Margaret Prang in an insightful analysis of the development of a national "public culture" through the emergence of nationwide business and religious elites; David Smith on the nation building and integrative strategies of the Liberal party in alliance with the national bureaucracy; and John Courtney on the role of national party conventions.

But the nation-centered thesis turns out to be hard to sustain and is indeed contradicted in many essays. As Stewart concludes, Sir John A. Macdonald's "calculating, comprehensive and thoroughly partisan" use of patronage to strengthen the national government ultimately failed because it did not foresee that "the new provinces could act as counterweights to the seemingly limitless power of the party that controlled the Commons" and because Canada "was too extensive geographically, too diverse ethnically, too fragmented economically, and . . . had no national ruling class to sustain

Macdonald's vision of central power" (pp. 42-43). This theme recurs throughout the essays. Thus, David Smith traces a sharp decline in the integrative capacity of political parties in the face of province building, the rise of policy concerns in provincial jurisdiction, and the importance of intergovernmental relations. Christopher Armstrong elaborates on the last point with an analysis of the growth of federal-provincial conferences as regional brokerage mechanisms displacing parties. Donald Blake shows how sharply divided national and provincial party systems are in British Columbia, while calling for a more aggressive assertion of federal power into the provincial-policy arena. John English shows that one integrative mechanism—the role of "Quebec lieutenant" for an English-speaking prime minister—has rarely been successful. Prang concludes that the importance of provincial governments, increased ethnic diversity, and increasing Canadian linkages outside the country have eroded the networks that earlier contributed to the "nationalizing of sentiment." Carty and Ward themselves conclude their analylsis of franchise laws by noting the "conflicts about the essence of Canadianness that lie at the heart of the political system" and the "lack of agreement on what constitutes a Canadian" (p. 77).

Thus, the book may be read less as a celebration of the success of nation building and more as a commentary on its relative weakness. Perhaps the key is that state building on the basis of the efforts of pragmatic, vote-maximizing, elitist politicians deploying the patronage instruments of the modern state is insufficient. It requires other qualities as well—and more attention than these authors pay to underlying social, economic, and ideological forces. Nevertheless, collectively, this is a stimulating, well-written, and useful book.

RICHARD SIMEON

Queens College

Corporatism and Political Theory. By Alan Cawson (Oxford: Basil Blackwell, 1986. viii, 174p. $45.00).

Twelve years after the publication of Schmitter's article on the "century of corporatism," his approach seems to have become something like the "orthodox" conceptualization of *corporatism.* As a synthesis of the recent debate, Cawson's book is, before all, a systematic exposition of this orthodoxy and a defense against the counterattack of the "pluralists." Cawson points out that *corporatism* does not necessarily supplant the pluralist "paradigm" but rather supplements it. This involves a restrictive definition of *pluralism* as an ideal type with a limited scope of applicability.

The other challenge to the emerging "orthodoxy" of corporatist theory is, of course, the structuralist version of neo-Marxism. Here the central issue is the concept of the state. Cawson places the corporatist concept of the state clearly into the Weberian tradition. In this perspective, in fields of welfare-state policies where the power base of the bureaucracy proves not sufficiently effective, state agencies enter into exchange relationships with "organizations representing monopolistic functional interests."

Compared to earlier writings of the corporatist school, the major shift in emphasis in Cawson's synthesis emerges in his distinction of three levels—macro-, meso-, and micro—of corporatism, according to the "scope of interest organizations" involved. Macrocorporatism intermediates interests aggregated on a class basis in peak organizations, and the level of state organizations involved is the central government of the nation-state, which has broad economic management as its "policy focus." Under this rubric, Cawson reports some of the cross-national studies of the impact of corporatism on policy outcomes and discusses Austria as an "ideal type." He presumes "that macro-corporatism is characteristic only of a few small countries, and that it has flourished especially in the context of economic growth in the post-war period" (p. 104). This, however, does not warrant the conclusion that the concept of corporatism is only of limited interest. But the specific category of *macrocorporatism,* if I understand the author's intentions, is of little use as an analytical category.

For Cawson, the potential is greater for *meso-* and *microcorporatism.* The latter he defines as "collaborative policy-making between a single firm and a state agency" (p. 76), distinguished from "agency capture" through industrial interests by a greater degree of "autonomy" of the agency (p. 121). The subsumption of such collaboration under the concept of *corporatism* is justified by the argument that large firms "aggregate" interests as well as organizations do. But how can this be reconciled with Cawson's definition of *corporatism* in terms of "organizations representing monopolistic functional interests" (p. 38). And whether there is anything on theoretical grounds distinctive about this subcategory remains obscure.

Obviously, the author's main emphasis is on *mesocorporatism* where, as he points out, organizations represent "sectoral as opposed to class interests" (p. 72). Cawson does not single out any countries as being specifically "mesocorporatist"—although he indicates that Britain has meso-, but only weak macro- corporatism (p. 136). The organizations at the level of sector—"be it industrial sector, public sector, consumption sector or whatever" (p. 73)—have restricted domains. Again, the author's ideas about what is theoretically distinctive in mesocorporatism are somewhat elusive. The "levels" of corporatism appear to be characterized by the predominance of specific policies. What is discussed in relation to the mesolevel is social and welfare policy on the one hand (p. 115) and industrial policy on the other (p. 113).

The latter is said to be "sectoral by nature" and to arise "when macro-economic instruments . . . fail to discriminate sufficiently between the different needs of various branches of industry" (p. 113). Thus, we see here the emergence of industrial restructuring as "negotiated sectoral policy" (p. 114). An important point seems to be that, whereas class-based macrocorporatism tends to be based upon "growth coalitions" depending on favorable economic circumstances, mesocorporatism is the form of "state intervention in industrial restructuring under crisis conditions" (p. 87).

The author thus seems to suggest that "macrolevel" corporatism is, first, a "fair-weather phenomenon" and, second, operates mainly through macroeconomic policy, whereas mesocorporatism is a crisis phenomenon and emphasizes sectoral industrial restructuring. Now it is certainly true that in some of the earlier contributions to the debate, corporatism was indeed discussed on the national (macro-) level and mainly associated to macroeconomic policies, in particular to incomes policy. (This was also the earlier perspective of this reviewer.) However, since then we have learned that in countries like Austria and Sweden, industrial policy and labor-market policy were central issues in the process of nationwide (and "class-based") "concertation" of peak organizations (and this already under conditions of economic growth). Hence, *macrocorporatism* should not be equated with negotiated "macroeconomic" policy. On the other hand, it is difficult to see that sectoral industrial policy is something specific to crisis. After all, the sectoral Economic Development Councils ("Little Neddies") in the UK had been established already in the sixties. The author wants to analyze "the impact of economic changes on corporatism" and to demonstrate that "what is involved is a shift in the level of corporatist practice, rather than an increase or decrease of corporatism as a whole" (p. 125). This may be an interesting hypothesis and a plausible one, at least as far as Britain is concerned. But it would be more convincing if the alleged correspondence were demonstrated with the aid of measurement (of "economic changes" or "shifts in level") and tested for cross-national validity.

GERHARD LEHMBRUCH

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The Faces of Justice and State Authority: A Comparative Approach to the Legal Process. By Mirjan R. Damaska (New Haven: Yale University Press, 1986. xi, 247p. $26.00).

Legal systems are often classified under rubrics like *Anglo-American* and *Continental* or *adversarial* and *inquisitorial.* Mirjan R. Damaska, a professor of comparative law at Yale Law School, reveals the inadequacy of such labels for cross-cultural study of the legal process. He offers instead an analytic framework that not only illuminates the world's diverse judicial systems but, more important for political scientists, demonstrates the impact of political ideas and institutions on legal structures and practices.

Damaska examines the administration of justice in terms of both the structure and the function of government. His structural focus produces two competing "ideals of officialdom." The *hierarchical* ideal, similar to the classic model of bureaucracy, reflects the centralized authority of Europe; whereas the *coordinate* ideal resembles the decentralized authority of the United States. These models help shape a nation's judicial process. In hierarchical regimes, the legal system entails specialized stages. Information is collected gradually and elaborate files are developed as cases ascend the procedural ladder. Authorities control proceedings strictly, allowing little role for litigants and their attorneys. Appellate review is routine before a decision is final. By contrast, legal proceedings in coordinate states are less integrated, testimony is oral rather than written, authorities delegate considerable procedural responsibility to actors outside the organization, and review from above is limited.

Governmental functions likewise affect the administration of justice. In *reactive* states, where government seeks only to maintain a stable order in which individuals are free to manage their own affairs, the legal system mainly resolves conflicts between competing parties. *Activist* states, on the other hand, limit individual freedom in order to mold society to a particular vision of the best order. Here justice facilitates implementation of state policies. While *conflict-solving* and *policy-implementing justice,* like *coordinate* and *hierarchical authority,* sound like sophisticated names for adversarial and inquisitorial proceedings, Damaska shows that they are independent of this traditional dichotomy because they influence both kinds of proceedings.

Combining these visions of authority and justice, the author surveys past and present legal systems to illustrate "the ways in which procedural features attributable to different goals of justice interact with features rooted in the character of officialdom" (p. 181). His conceptual scheme is particularly useful for understanding recent changes in the U.S. judicial process, such as the rise of public-interest litigation. "Much as the *functions* of the contemporary American state are a mixture of activist and reactive impulses," Damaska argues, "so the actual *structure* of the state apparatus—especially the administration of justice—is a hybrid of bureaucratic and prebureaucratic forms" (p. 234).

Nevertheless, he understates how bureaucratic our legal system is (pp. 44-46, 231-34). In terms of decision making as well as administration, the U.S. legal process has become a large-scale, complex organization displaying many of the hallmarks of Max Weber's model of bureaucracy: shared values, beliefs, and expectations; loyalty to the organization and its goals; consensual decision making by mutually dependent and supportive work groups; routinized operating procedures; and secrecy and hostility toward outsiders. A good example of this bureaucratization is the collaboration among police, prosecutors, defense attorneys, and judges in criminal cases.

Although the book makes surprisingly little use of political-science research, given its recognition of the importance of politics in understanding law, it is nonetheless a novel exploration of the connection between justice and state authority. It places much of what we already know about the administration of justice in a fresher perspective while identifying patterns and suggesting meanings not appreciated before. Damaska's elegantly written and flawlessly organized work is therefore a valuable contribution to comparative analyses of the legal process, showing that study of the relationship between law and politics is far from exhausted.

GERARD J. FITZPATRICK

Ursinus College

Political Socialization in the Arab States. Edited by Tawfic E. Farah and Yasumasa Kuroda (Boulder, CO: Lynne Rienner, 1987. xiv, 215p. $25.00).

Jo-Ann Hart's valuable introduction to this collection of survey essays ends with Roberta Sigel's self-criticism that *political socialization* "is a misnomer for what we study because we study what children have learnt . . . not how they have learnt it" (p. 15). The great majority of these essays have not fallen into that trap. This leaves the reader with both an interesting and relevant source, because in the Middle East the function of political-socialization agencies is more that of system creation than of system maintenance. However, in the Middle East, system creation and political legitimacy are inherently linked to the ongoing effort to create a common communal association and national identity. These communal associations have taken two "modern" forms since the collapse of the Ottoman Empire, those of state and Arab nationalism. "Pan-Arabism," as much a socioeconomic rallying point for the disenfranchised under the old regimes as it was an antiimperialist and anti-Israeli nationalist movement, threatened "parochial" state nationalism under much of Egyptian president Nasser's rule but faltered from the mid-1960s onward until it was debilitated by the debacle of the 1967 war and Nasser's death in 1970. The Arab state, sometimes no more than a dynastically and or bureaucratically led collection of religious and ethnic groups, families, and tribes, survived much of the Arab system, but since 1979 it has come under a new ideological assault from Shiite Islamic fundamentalism.

Identity, personal or national, is a dynamic process and the history of a people's self-identification is the history of both their objective situation and their self-conception within that situation. The best articles in this collection attempt to identify the most relevant socializing agents in Arab society and, at the same time, place the respondents, families, schools, peers, and national institutions within the region's political context. These include two focused on Kuwait: one by Tawfiq Farah and the second coauthored by Farah and Faisal S. A. Al-Salem. These examine the issue of efficacy and succeed in decoupling the theoretical link between interpersonal trust and trust in an individual's government and political system. Trust in a system, they persuasively argue, is not a subjective or psychologically induced trait but emerges, when it does, from experience and successful interaction with the system. While traditional social units, such as the family, frequently provide the required access to the material and status benefits that the government provides in Kuwait, the result is that for male Kuwaiti citizens of all classes, the system is responsive due to the small size of the population and the wealth and the welfare-nature of the system.

This "instrumental" approach to political socialization is also found in Mark Tessler and Linda Hawkins's study of Tunisia. Tessler and Hawkins find some of Joel Migdal's earlier conclusions (*Peasants, Politics, and Revolution* [Princeton: Princeton University Press, 1974], 12, 20) an aid in understanding the oft-found contradictions in surveys regarding modernization. One is that "individuals often change some patterns and commitments, but not others" and the other is that "self-interest largely determines whether an individual accepts or rejects new cultural patterns to which he is exposed" (p. 108). The results of the Tunisian survey lead the authors to the conclusion that "If acculturative experiences [including education] are unaccompanied by increased socio-economic status, they do not lead to variations in psychological or political outlook" (pp. 117–18). However, these findings tend to differ from the results of the studies in the Gulf States in one important respect. While the respondents in Tunisia who experienced greater socioeconomic mobility changed their political attitudes in the direction of greater "participant citizenship," the surveys of the Gulf States found that the family and religion—and not the state—remain the prime foci of loyalty even for upwardly mobile males.

This forces Ahmed Dhaher to conclude from his survey in "Culture and Politics in the Arab Gulf States" that while the material and ecological setting has been rapidly changing, the Arab mind has not "kept pace": "The students in the sample have come to expect modernity in their materialistic needs while maintaining a traditional mental outlook, apparently unaware of the contradiction" (p. 76). However, young Gulf women and a majority of respondents of both genders from Bahrain tended to be much more highly politicized in general and more closely identified with the "liberation of Palestine" than the aggregate, leading Dhaher and Maria Al-Salem (in "Women in the Gulf") to speculate that those individuals who see themselves as most repressed in their own social setting have had their consciousness raised to the point of at least verbalizing a commitment to the liberation of other Arabs.

The two surveys on Palestinian identity and political loyalty indicate that traditional social structures have the potential of serving as the vehicle for modern political mobilization and activity. Tawfiq Farah's "Learning to Support the PLO" and Rosemary Sayigh's "Sources of Palestinian Nationalism" should be read together. While very different in approach and representative sample, each tends to find the family as the major socializing agent for

Palestinian consciousness and identity among the young, rather than the PLO-run schools or the respondents' peer groups. One might theorize that due to the stateless nature of the Palestinian people, Yasir Arafat (the personification of the national expression), the family, and the "homeland" have become linked in the Palestinian psyche.

One overall criticism of this collection is that many if not most of these surveys took place at the earliest stages of, or prior to, the 1979 revolution in Iran. This event was riddled with nationalist, religious, and socioeconomic dimensions and has greatly influenced the altogether unresolved issues of identity and legitimacy throughout significant parts of the Arab world. An updated series of surveys can easily rectify this shortcoming.

STEWART REISER

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Decolonization and the State in Kenya. By David F. Gordon. Special Studies on Africa (Boulder, CO: Westview, 1986. 266p. $21.00).

In strictly economic terms Kenya has to date been among the most successful African countries during the first postcolonial generation. At the same time, after having survived one of the more traumatic colonial experiences among African former colonies, Kenya has been among the most open to foreign and domestic private investment of any country on the continent. The conventional wisdom among "mainstream" economists has been to see Kenya's economic progress as a direct consequence of its policies and to say to other economically struggling African countries, "Go thou and do likewise." Political economists of a more radical persuasion have seen Kenya's fate as the result of the changing character of international capitalism, disagreeing on whether this has (Colin Leys, *Underdevelopment in Kenya* [Berkeley: University of California Press, 1975]) or has not (Nicola Swainson, *Development of Corporate Capitalism in Kenya* [Berkeley: University of California Press, 1980]) produced dependency. They agreed in challenging earlier analyses emphasizing the salience of major local political developments and responses to them (e.g., the Mau Mau Emergency) in shaping Kenya's future course (e.g., Carl Rosberg and John Nottingham, *Myth of "Mau Mau"* [New York: Praeger, 1966]; George Bennett, *Kenya, a Political History* [London: Oxford University Press, 1963]; and George Bennett and Carl Rosberg, *Kenyatta Election* [New York: Oxford University Press, 1961]). Those centering on the conjunction of land-transfer schemes and political decolonization differ over whether local European-settler economic interests did (John Harbeson, *Nation-Building in Kenya* [Evanston, IL: Northwestern University Press, 1973]) or did not (Gary Wasserman, *Politics of Decolonization* [New York: Cambridge University Press, 1976]) dominate the transfer of power and significantly shape Kenya's future. Underlying these debates is the question of how much relative autonomy the colonial regime enjoyed and which constituencies were most effective in limiting that autonomy in their own interests.

David Gordon's *Decolonization and the State in Kenya* is a refreshing new contribution to this ongoing debate. His thorough and balanced review of the rise and demise of the Kenya colonial order facilitates, if it does not resolve, the foregoing debates. Gordon's argument is that the colonial "state," if that's what it was, was too "diverse and ambiguous [a] collection of parts" to have dominated, or been dominated by, any single interest or underlying economic trends or to have dominated the new regime after its formal demise. There is ample room, he believes, for the play and interplay of political strategy in policy formation and execution.

Gordon builds a strong case for the importance of the political that should be taken to heart by economic and structural determinists in our midst. Yet the "indigenization of control," both economic and political, does not by itself, in my view, disprove the Kenya-as-neocolony case. The problem centers on the question of "control," the many complexities of which it is beyond the scope of Gordon's book to explore fully. Neocolonialism is not incompatible with such processes, nor is it incapable of reappearing in new forms as societies appear to change. At the same time, Gordon's long historical view of the interplay of political economic developments is important. That same perspective applied to the first quarter century of Kenya's independence will show, I think, that neocolonialism is not forever present, a point that Kenyans have begun and will continue to demonstrate.

JOHN W. HARBESON

City University of New York

Forming Economic Policy: The Case of Energy in Canada and Mexico. By Fen Hampson (New York: St. Martin's Press, 1986. x, 161p. $27.50).

Hampson uses energy policy in Canada and Mexico to demonstrate how and when governments act in the long-term national interest, a behavior contrary to the pluralist view that elites respond to "micropolitical" pressures. According to the "statist" model, perceptions of crisis mobilize leaders to create policies meant to preserve stability and legitimacy at the risk of alienating important societal groups.

The price and availability of energy was perceived widely as a crisis in the 1970s. Canada and Mexico joined other governments in seeking policies to cope with problems of security and equity. Both faced the trade-offs between blessings and costs of expanding domestic production in response to rising world oil prices. They shared the opportunities and problems of land borders with the United States and its oil deficits. Was there "politics as usual" in these two countries? Did the state act autonomously to formulate policies to reverse the damaging effects of past concessions to micropolitical pressures?

In Mexico, the problem with oil lay in the rapid acceleration of production that was meant to capitalize on the rising world price of oil. While success in oil production and exports fostered economic growth, there were the destabilizing effects of drawing scarce capital, increasing the foreign-debt service, milking resources from other sectors, and fueling inflation. President Lopez Portillo capped PEMEX production in 1979 in order to reverse these trends. The effect of this policy was short-lived, as world oil prices fell in the 1980s.

Unlike Mexico, Canada was incapable of being a net exporter of oil. Conflict centered on eastern Canada's vulnerability to rising imported-oil prices and western Canada's frustration over being denied the producer's right to sell at market prices. In the National Energy Program (NEP) of 1980, the Liberals ended an oil-price freeze, forcing higher gasoline prices on eastern consumers, and also imposed a wellhead production tax to capture revenues for a strapped federal government. The "made-in-Canada" price fell victim in the 1980s to the downward reversal in oil prices, making a judgment on the NEP difficult.

Acute crises resulted, the author argues, from concessions to parochial interests. In Mexico, decisions to expand production and shelter prices were meant to hold consensus among the dominant elements—labor, business, agrarian—of the revolutionary coalition. In Canada, the Liberals wanted to avoid alienating pivotal constituencies in central Canada by controlling oil and gasoline prices.

These two cases show, Hampson argues, that pluralist and statist theories are actually complementary. Parochial interests usually prevail, but under conditions of widely perceived crisis, elites rise above the pressures to secure the national interest. The author acknowledges a weakness of this approach. Retrospective analysis allows the observer to define a crisis and describe cause-and-effect circumstances.

The paradigm also does not allow the reader to independently categorize policy that is merely the constellation of private, parochial interests and policy that meets an unequivocal definition of the national interest. In the case of the NEP, some critics have argued that its publicized features of security, fairness, and efficiency were transparent as energy policy and that it was little more than a confirmation of the Liberals' view of the virtue of economic centralization. "Canadianization" of the oil industry was intended to help secure the Liberals' electoral base in central Canada. In Mexico as well, the same arguments about national salvation made in defense of Lopez Portillo's decision to cap PEMEX production could as easily be applied to the previous decision to mobilize production—the action that supposedly produced the crisis.

The statist model does not, in practice, provide a useful distinction between the objectives of the "state" as the embodiment of sovereign interests and those of the government as the resultant vector of the society's dominant interests. In Mexico, what distinguishes the preservation of the interests of the state from the interests of the revolutionary coalition? In Canada, to what extent was the NEP the ideological vision of senior civil servants, and to what extent pure power politics?

Notwithstanding these reservations, Hampson's book is a good effort that calls attention to the need for more studies to explore the convergence of statist and pluralist interpretations.

LAUREN S. MCKINSEY

Montana State University

Guyana: Politics and Development in an Emergent Socialist State. By Kemp Ronald Hope (New York: Mosaic, 1985. 136p. $19.95, cloth; $12.95, paper).

Trinidad and Tobago: Democracy and Development in the Caribbean. By Scott B. MacDonald (New York: Praeger, 1986. ix, 231p. $31.95).

Revolutionary Grenada: A Study in Political Economy. By Frederic L. Pryor (New York: Praeger, 1986. xx, 395p. $45.00).

Militarization in the Non-Hispanic Caribbean. Edited by Alma H. Young and Dion E. Phillips (Boulder, CO: Lynne Rienner, 1986. ix, 178p. $20.00).

The case studies of Guyana, Trinidad and Tobago, and Grenada by Kemp Hope, Scott MacDonald, and Frederic Pryor, respectively, and the contributions in the volume on militarization edited by Alma Young and Dion Phillips provide a variety of assessments on the nature of the problems faced by the non-Hispanic Caribbean since the attainment of independence and how different governments have responded to them. In one form or another, the four books focus upon the forces, both external and internal, that affect the chances for countries in the region to preserve and maintain democratic practices while at the same time formulating and implementing effective programs of economic development.

There is a general consensus by all the authors as to a growing militarization in the region as a whole and in the individual countries in response to domestic political concerns for order, stability, and regime survival and as a result of the intensification of international Cold War contentions in the Caribbean and Central America. There is general agreement that militarization can have a profound negative effect upon the chances for sustaining democratic practices or of achieving self-sustaining economic development.

From Hope's case studies of Guyana and Pryor's of Grenada, the adoption of "Third World socialism" appears to bring with it the certainty of economic crisis and stagnation as well as kill any chances for the preservation and maintenance of democratic political practices. Pryor does not see these outcomes as inherent in socialism per se. Rather, the explanation has to do with how this ideology has become fundamentally and aberrantly altered in its transposition to the Third World. In his fascinating and highly convincing analysis, he concludes, with reference to his own set of economic, political, and sociocultural criteria, that, like Grenada, most Third World countries identified as Socialist would rank very low on a Socialist scale. The exceptions to these criteria are in their adoption of the vanguard party, in their international support for Socialist nations, and in their ideological rhetoric. These three characteristics, he seems to be saying, constitute the entirety of the phenomenon of Third World socialism.

In his case study of Guyana, Kemp Hope provides an example of how these three elements can easily be employed to ensure and underwrite regime survival. There, the notion of the vanguard party was used to secure the centralization and personalization of power, to underwrite the conversion of the state administration into an institution of patronage, and to justify the development of a system of coercion and control. In addition, both ideological rhetoric and a foreign policy based on international support for Socialist nations were opportunistically employed to elicit economic assistance and strategic political support from the Eastern bloc and from progressive Third World nations.

What occurred in Guyana constitutes the essence of what Pryor calls "foreign-aid socialism" characterized by a complete dependence upon foreign grants and concessionary loans. The intention of those adopting this form of socialism is to employ such loans and grants for promoting economic development and supporting efforts at structural change.

The case of Grenada highlights the problems that inhere in such a strategy. Pryor shows how the Grenadian leadership became preoccupied with establishing their "Socialist credentials" in the international arena while lacking a fundamental and sophisticated understanding of socialism itself. This invariably leads to the failure of developmental planning, an outcome also observed in Guyana by Hope, who attributes it to ideological dogmatism in the Guyanese government's approach to development. For Pryor, however, such failure has to do with the inability of governments adopting foreign-aid socialism to devise a consistent and integrated development program. This is because of their absolute reliance upon a foreign-assistance package, the contents of which are entirely determined by what foreign donors are willing and able to provide and which constantly change over time. To this is added the problem, endemic to all Third World countries, of the severe shortage of skilled personnel with the requisite levels of training and administrative acumen to implement a program of Socialist economic development. In Grenada, the inevitable developmental failures were hidden by an effective propaganda campaign directed at the domestic population, and more successfully at the international, particularly Socialist, community.

Pryor rejects almost entirely any suggestion of foreign meddling in the affairs of Grenada as an explanation for the political and economic crises of the Peoples Revolutionary Government (PRG). By contrast, the contributors to the volume on militarization, with the exception of the Seduc-Dahlberg article on Surinam, emphasize the realities of U.S. regional hegemony and the intolerance, particularly of its present administration, for any progressive regimes in the region. Such an emphasis is also evident in Hope's case study of Guyana. He suggests that the failure of the Guyanese regime to recognize this reality has distanced it from the "new Caribbean leadership." It is this very failure that explains the economic and political crises affecting that country.

In the volume edited by Young and Phillips, Boodhoo sees the escalation of militarization during the PRG regime in Grenada as the continuation of a trend toward state violence that began in the late 1940s. The dramatic military buildup after the regime came to power had, as its initial cause, the undoubted fear of U.S. military intervention or U.S. covert support for Eric Gairy, the ousted former prime minister. It was this fear that drove the PRG to make military agreements with the Soviet Union, Cuba, and North Korea thus driving the country much more closely into the arms of the Communist bloc.

The growing militarization in the region is related, by all the contributors to the volume, to the erosion of democratic institutions. The contributions by Danns on Guyana and Seduc-Dahlberg on Surinam both deal with the use of the military as the primary prop to increasingly unpopular regimes. In the case of Grenada, according to Boodhoo, the PRG's program of militarization and its failure to hold elections contributed to its eventual collapse. In the introductory article by Phillips and Young, in the latter's article on Belize, and in the overview article by Watson, the source of the threat to democracy is clear: the efforts by the United States to assert or reassert its hegemony in the region. To this is added the fear of mass radicalization by the current crop of Caribbean leaders in the face of an escalating economic crisis. The renewed willingness of the United States to prop up pro-U.S. regimes and to provide them with military assistance and the threat to what Watson sees as petit bourgeois domination are acting in tandem, according to him, to "Central Americanize" the Caribbean. By this he means the proclivity in that region for right-wing, U.S.-sponsored, authoritarian régimes. A variation on this theme is provided in Alma Young's contribution on Belize. There, growing militarization has been sponsored by the United States in view of what it sees as the strategic importance of the country in the politically volatile Central American region.

What are the necessary prerequisites for democracy in the non-Hispanic Caribbean? Scott B. MacDonald attempts to provide an answer by examining the "successful experiment" of democracy in Trinidad and Tobago. This he attributes to the emergence of a dominant and politically unified middle class, which provided the basis for the establishment of a system of state-directed capitalism. MacDonald sees episodes of lower-class mobilization as anathema to democracy if only because they lead to racial fragmentation and are rooted in radical ideology. He traces middle-class moderation to "acceptance of European social attitudes" and, more recently, to the importation of middle-class ideas as a result of the military presence of the United States in the country between 1941 and 1960. MacDonald thinks that the two challenges to Trinidad's democracy are lower-class radical mobilization and the breakdown of middle-class consensus. These two combined in the early seventies to produce an authoritarian tendency that was turned back in the wake of the post-1973 oil boom that laid the framework for the strengthening and consolidation of "democratic capitalism." The question that needs to be answered is why did middle-class domination result in democratization in Trinidad and Tobago when, from the accounts of all the authors considered, it appears to be the crux of the problem of militarization, underdevelopment, and growing authoritarianism throughout the region?

PERCY C. HINTZEN

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Black Politics and Urban Crisis in Britain. By Brian D. Jacobs (Cambridge: Cambridge University Press, 1986. viii, 227p. $44.50).

The title of this book promises a comprehensive analysis of the role of Asian and Caribbean people in political action and protest in British cities. Jacobs dismisses on various grounds most extant writing on the political participation in Britain of people of Asian and Caribbean origin. He sets himself the task of mapping the pattern of political activity of, the parameters of political debate amongst, and the impact on public policy (particularly urban questions) of, this section of the British population (p. 6). His central claim is that the leaders of Asian and Caribbean communities have been slowly integrated into various forms of cooperation with local and national governments as the latter have attempted to execute policies intended to reverse the disadvantaged position of Caribbean and Asian people, particularly in Britain's urban areas.

Jacobs makes a virtue of his descriptive intentions but thereby lapses into empiricism. Uncritically, he utilizes the "race relations" problematic and abstracts "black politics" from its class context, this being legitimated post hoc by his claim that he is concerned with policy rather than theoretical issues (e.g., p. 5). It is mistaken to limit the conception of political activity of Asian and Caribbean people to include only forms of activity within or exclusively on behalf of the Asian and Caribbean communities, although this conception follows from regarding these populations as shaped by, and only concerned with, "race."

Jacobs is therefore silent about Asian and Caribbean electoral behavior, trade union membership and participation, involvement in industrial disputes, and political-party membership and participation. An integrated analysis of these forms of political activity would have allowed him not only to realize his stated objectives but also to comprehend theoretically the process of political integration he describes. There is no reason to doubt that a process of political incorporation intended to achieve, or resulting in, the neutralization of dissent is occuring in Britain, but its scale and significance require a more thorough analysis of Caribbean and Asian political activity than Jacobs offers.

There is an additional empirical limitation. Having argued that Asian and Caribbean political organization has been localized, the only example he cites is that of Wolverhampton, where his focus is almost exclusively upon the Community Relations Council (CRC). Because the formation of CRCs led to the simultaneous creation and incorporation of Asian and Caribbean leaderships, this focus certainly sustains Jacobs's thesis. But I could accept it more easily if Jacobs had demonstrated that this organization was the primary focus for Asian and Caribbean political activity in Wolverhampton (something that could only be done by means of a more systematic and extensive survey of the political activity of Asian and Caribbean people in Wolverhampton, and if a similar pattern were revealed in other towns and cities. Case studies of single cities can be extremely useful, but in the absence of supporting evidence or of an argument and evidence that they can be considered to be typical or representative, one must hesitate over the generalization of the findings to other contexts. Elsewhere in the book, key assertions are not supported with evidence from Wolverhampton or from any other city. For example, we are told that young Caribbean and Asian people have begun to use state and private assistance to establish themselves as part of the growing petite bourgeoisie, but we are offered no evidence or source (p. 167).

Jacobs's contextualization of his analysis is weak, particularly his historical overview of British state policy regarding immigration. He describes state practice as pragmatic (p. 22) but can only sustain this interpretation by ignoring detailed evidence to the contrary. For example, he ignores the fact that the Labour government of 1945–51 recruited non-British refugees to resolve labor shortages while simultaneously but secretly considering methods by which to prevent British subjects from entering Britain because they were "colored." He ignores the method by which a Labor government withdrew in 1968 the right of British passport holders to enter and settle in Britain if they were "colored" and the formalization of that device by a Conservative government in the Immigration Act of 1971, an act that increased by several million the number of "white" people with the right to enter and settle in Britain. There is a consistency in the actions of the British state on immigration that supports the assertion that it has systematically institutionalized racism.

These weaknesses are not offset by the positive features of the book. It usefully describes central-government initiatives and the emergence of private-company strategies to deal with urban decline and political protest during the 1980s. And the hastily added Postscript describing the political disturbances of 1985 suggests that these various initiatives have not resolved the major structural determinants of political protest and violence. It is also useful to have the recent debate about the role and effect of the Commission for Racial Equality summarized and clearly set out. But the author has not realized his objectives, and in the absence of a more thorough survey of Asian and Caribbean political activism, it is difficult to evaluate the extent and significance of the process of political integration that Jacobs describes.

ROBERT MILES

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Legal Traditions and Systems: An International Handbook. Edited by Alan N. Katz (New York: Greenwood, 1986. viii, 450p. $55.00).

This is a large and physically impressive volume that treats a limited set of subjects for a large, implicitly worldwide sample of legal systems. It consists of an introductory essay by the editor and 18 substantive chapters (seven contributed by the editor), each dealing with the legal traditions and systems of either a single nation or a group of nations. The very brief (five-page) editor's introduction lays out the outline followed by the authors of the substantive chapters. Each opens with a discussion of the "historical development" and organization of the legal system. Each then turns to the "personnel of the law," especially the "recruitment and training of those entering the profession" and mobility within the profession and its relation to judicial selection and promotion. Finally, the outline calls for each chapter to discuss "the public's perception of the legal system and its personnel and the role played by the courts in the legal system studied" (pp. 2–4).

It is difficult to know what to make of this outline. Aside from an attempt to link the first two elements of the outline to a definition of legal tradition offered by Merryman and Clark, the editor makes no effort to develop a rationale for its use. Perhaps it is seen as an obvious or self-evident organizing tool for the production of "handbook fare." But the potential difficulties that lie in its lack of development are clearly seen in the editor's two-page discussion both of the evidentiary problems accompanying efforts to assess "the public's perceptions of the legal system and its personnel and the role played by the courts" and of the meanings atached to the "role played by the courts." At a minimum, assessment of the latter is said to involve assessment of the judicial independence and exercise of judicial review by the courts studied. The discussion that ends up being mandated by these outline topics is truly global and is inevitably muddled by use of available, selectively chosen evidence documenting the courts' "roles."

Each of the substantive chapters conforms fairly well to the editor's outline, especially given the broad and ambiguous tasks it mandates. In some cases, this leads to a very dry, formal-legal discussion with no great depth as the chapter authors' attempts to fill in the outline fall back on the evidence of constitutional and statutory provisions in the absence of better quality evidence.

Although there is no explicit statement of the book's intended geographical coverage, the chapters included deal with the vast majority of the world's legal systems in some way or another. Nine chapters deal with the legal traditions and systems of individual nations: the Federal Republic of Germany, France, Italy, Japan, the People's Republic of China, the Soviet Union, Spain, the United Kingdom, and the United States. Four chapters treat small groups of nations that are united by some fairly closely shared historical or cultural backgrounds: the Benelux nations; Canada, Australia, and New Zealand; India, Pakistan, and Bangladesh; and Scandinavia.

The remaining chapters report on the rest of the world considered as more or less standard geographical-cultural divisions: Africa (actually black, sub-Saharan Africa), Eastern Europe, Latin America, and the Middle East. These regions contain from some (Eastern Europe) to considerable (Southeast Asia) variety in political systems, national histories, cultures, languages, religions, and so on.

The "great-powers" bias of the book's composition is striking to any student of contemporary world politics. One might easily accept the presence of separate chapters on the legal systems of China, the Soviet Union, and the United States, three of the world's most populous and powerful nations. But it is less easy to defend including separate chapters on the Federal Republic of Germany, France, Italy, and Spain while lumping India with its undemocratic neighbors, treating Brazil as merely one of six examples of Latin American legal systems, and paying no separate systematic attention to Africa's largest nation, Nigeria. The point is not that the chapters on the Western Eurpean nations are uninformative or poorly done; it is that even the larger and potentially more powerful nations in Third World continents get relatively short shrift in the design of *Legal Traditions and Systems.*

Part of the reason for this great-power bias is a short supply of adequate sources for many Third World legal and judicial systems. But that can be only part of the problem: sources on the Indian legal system and courts are voluminous, for example. To fulfill their assignments within the apparent space limitations, authors of the regional chapters are forced to make some difficult coverage choices. Some, Harvey Feinberg in his chapter on Africa, for example, choose to pitch their discussions at a very high level, seeking broad generalizations and using concrete examples to illustrate their points. Others, Lee Epstein, Karen O'Connor, and Diana Grub in their chapter on the Middle East, for example, choose to concentrate on specific nations (Egypt and Israel, in this case) as regional examples of apparent or demonstrated relevance.

Overall, this work will be of only limited value to political scientists. Its focus on legal traditions and systems moves it away from the mainstream concerns of public law and judicial politics, along with judicial policy-making and judicial behavior. Its coverage, though broad, is not broad enough to make it a comprehensive reference tool. But it is broad enough to make much of the discussion formal and shallow. It is in discussing the roles of the courts that the handbook chapters most frequently touch closely on matters of central concern to political scientists. It is also in these discussions that the gap is greatest between what the authors say and what they can say with confidence because it is based on firm evidence.

C. NEAL TATE

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Politics, Economics, and Welfare Reform. By Leslie Lenkowsky (New York: University Press of America, 1986. vii, 207p. $12.75).

This book gives an account of the effort to implement versions of negative income tax in Britain and the United States. "The story of FAP, FIS, and the tax-credit plan," we are told, "suggests that whatever the future brings in Britain and the United States will emerge from the interplay of historical, political, economic, and cultural factors in each nation" (p. 188). Dr. Lenkowsky clearly does not like to go "out on a limb." And, barring the intervention of Shirley Maclaine's extraterrestrials, the future is unlikely to let him down. Generally, this book is more compelling in its description than in its analysis.

This comparison of attempts to implement negative income tax is presented as an example of "surprising convergence," as a method for explaining the genesis and failure of policy proposals, and as a means of arriving at general conclusions about the two political systems. The book's contributions lie elsewhere, however. They may be found in the discussion of the idea itself as a serious alternative to the present structure of social policy.

Dr. Lenkowsky claims that "negative income tax was an idea that could appeal to liberals and to conservatives, to those who liked the 'welfare state' and to those who valued individualism" (p. 174). The most important reason for the "surprising convergence," he argues, was the central role played by experts in advancing the idea. As a general concept, it is true that negative income tax is ideologically neutral. But as soon as it is given a specific structure of tax rates and benefit rates it reflects the values of policy makers. Targeted towards poverty, negative income tax offers individually tailored help for the "truly needy" that substitutes cash for the complicated network of existing policies, without diminishing work incentives or damaging the operation of the market. These are the advantages Milton Friedman mentions, and from this perspective it is not really surprising that such a policy, consonant with a particular market-oriented vision of the welfare state, should appeal to both Richard Nixon and Edward Heath. (Perhaps more surprising is that the more self-consciously Friedmanite Margaret Thatcher and Ronald Reagan, to an extent, should have neglected the idea.) It had become a right-wing idea, associated with right-wing experts, and so appealed to right-wing governments. Actually, even as a general concept, there is an ideological content to negative income tax insofar as it is viewed as a complete solution to welfare problems, without regard for health policy or full-employment policy, for example.

Aside from his argument about negative income tax being a rational and attractive idea propagated by experts, Lenkowsky suggests that both countries share similar welfare states, even in health policy (p. 171), and that these similarities in policies and values "led Britain and the United States toward the negative income tax" (p. 175). I am not aware of any comparative work on the welfare state that would support this claim of similarity.

From this comparison of welfare states we move to the equally shaky ground of a comparison of whole political systems. British government deals badly with complex issues, due to the "often unrecognized restraints of Parliamentary government" (p. 177). Briefly, a Prime Minister, "before making substantial changes," needs a consensual manner and the cooperation of ministers, backbenchers, civil servants, the opposition, and relevant interest groups. Lenkowsky should tell this to Mrs. Thatcher.

The book is better when describing the progress of the policies. While saying that the negative income tax failed because it ws too sophisticated, Lenkowsky also provides evidence that the idea had its difficulties. In his account of the work of the Senate Finance Committee, he describes how Senator Williams pointed out the "notch effects," where in some circumstances a rise in earnings would result in a loss of income as a result of the proposed policy. In addition to similar objections, in Britain there was also the problem of the low take-up rate. Those eligible for the Family Income Supplement did not always receive it. Lenkowsky apparently relies on government figures in discussing this problem. Curiously, while Peter Townsend appears in the impressive interview list at the end of the book, his *Poverty in the United Kingdom* (Berkeley: University of California Press, 1979), which comes up with significantly higher figures, is not cited. Also omitted from this book is an index.

It does have on its cover, however, very favorable comments from two participants in the policy-making described, Daniel P. Moynihan and George P. Shultz. Moynihan says that Lenkowsky "has mastered that most elusive art of comparative politics." His art has eluded this reviewer.

NEIL J. MITCHELL

University of New Mexico

Sozialdemokratische Verfassungstheorie in der Weimarer Republik. By Wolfgang Luthardt (Opladen: Westdeutscher Verlag, 1986. viii, 194p. 32 marks).

Luthardt's short book on social democratic theories of the Weimar constitution epitomizes a decade during which younger German social scientists have rediscovered and reassessed the achievements of some previously neglected German exiles from Nazism—a number of Social Democratic thinkers trained in the law, including Hugo Sinzheimer, Herman Heller, Hans Kelsen, Ernst Fraenkel, Franz L. Neumann, and Otto Kirchheimer. Many U.S. political scientists also will find a lot to think about in Luthardt's analysis and assessment of these sophisticated attempts to interrelate a sociologically realistic democratic constitutionalism with a design for radical social change based on labor and proceeding through law. The most recent turn in much contemporary discussion, moreover, means that U.S. political scientists will not be discouraged by the fact that this political theorizing has a marked juristic component. The political sources, uses, and limitations of law have become central themes in present-day inquiries into the supposed "crisis" of the welfare state, as they were in Weimar.

Luthardt states the central question confronting almost all of his protagonists as one of how Socialists, who consider a progressive dismantling of capitalism a necessary condition for the maintenance and extension of democracy, can subordinate their conduct to a legality defined by a parliamentary democratic constitution that guarantees their opponents ample chances for making political use of their massive social power. This formulation connects with the predominant theme in the self-criticism by Neumann and others after 1933, that the Social Democratic strategy was vitiated by excessive "legalism." According to Luthardt, the response towards which his theorists tended is best summarized in the conception of "collective democracy" adumbrated by Ernst Fraenkel in 1929, at the time of the last Socialist-led coalition regime and before the impact of the Great Depression.

Collective democracy is a theory of the Weimar constitution that stresses the intrinsic value of its democratic political forms as an alternative to absolutist violence, that affirms the indispensability of compromise, and that locates the dynamics for progressive social change in the legislative and social development legitimated (and in part initiated) by the constitutional provision for parliamentary sovereignty and social rights. The three labor lawyers, especially—Sinzheimer, Neumann, and Fraenkel—elaborated the third of these points. For them the institution of a "parity" constitution, parallel to the political one, was of the highest importance. This was to be a constitution that would give new contents and new impulses to legislative politics, as witnessed by the transformation of property law through labor law (especially through the operations of collective agreements) and the transformation of administrative and judicial agencies through the incorporation of organizationally grounded social judgments and negotiations, counteracting statism and class justice.

Unlike many critics, Luthardt does not belittle the theory of collective democracy when he turns to an analysis of the "confining conditions" that obviated its designs in Weimar, especially under the pressure of economic disaster. After 1930, he points out, the Social Democratic conception was rendered ineffectual. The parliamentary legislative state was confronted by a parallel and superior legitimacy grounded in statist and plebiscitarian elements in the constitution, a legitimacy that the courts used to devise a "legality" fatally limiting the political and social relevance of participation by the labor movement. The non-socialist partners in the political and social processes of compromise abandoned the earlier common ground. The incipient social constitution effectively collapsed. When the democratic labor movement opted for "legality" between 1930 and 1933, according to Luthardt, the substance of legality no longer corresponded with the constitutional design upon which these thinkers had not unreasonably pinned their hopes. All of them recognized this (although Luthardt singles out Kirchheimer as the most astute), but none of them formulated alternatives beyond rhetorical statements of defiance or tragic acceptance of what they could not change. Luthardt's somewhat eccentric inclusion of Kelsen in the study, despite his distance from most of the elements of "collective democracy," clearly signals Luthardt's own resolution of the question of what is to be put first if the constitutive question of democratic socialism cannot be successfully answered in a socialist way.

Luthardt does not evade the question whether the failure of the Social Democrats' project in Weimar makes their conceptions more than a deterrent object lesson. Neumann and Kirchheimer thought so themselves. But Luthardt draws on the past generation of remarkable historical scholarship to present a diagnosis of that failure that is far more nuanced than the shocked self-judgments of the defeated participants or the facile stereotypes that grow out of them. The result is a more qualified and open response to the structural and strategic thinking of the reformist Left. He thinks that Weimar may still have something to teach us. Nothing can turn the history of Weimar social-democratic speculations into a success story. But it can be productively demythologized and rendered useful for discriminating comparative study and reflection by political scientists. Luthardt's book is a very useful contribution to the latter undertaking.

DAVID KETTLER

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Supreme Courts and Judicial Law-Making: Constitutional Tribunals and Constitutional Review. By Edward McWhinney (Dordrecht, The Netherlands: Martinus Nijhoff, 1986. xvi, 305p. $67.00).

Edward McWhinney, prolific writer on world judicial systems, has written another book on the role of courts in the policy-making process. Here, in the context of several national legal systems, he looks at the variables related to the role of *judicial review*, which he defines as "Court practice in applying constitutional norms to scrutinise executive and legislative actions" (p. xiii). Because of the influence of the U.S. judicial system, this is actually a study of the dissemination of the U.S. principle of judicial review into other legal systems of the world.

McWhinney attributes the rise of modern-day judicial review in large part to the prestige and influence of the United States in the postwar world. He notes that the example provided by U.S. independence from an imperial power also made the United States a likely model for judicial review. Moreover, for former Axis nations, adopting a new constitutional system was seen as a way of embracing democracy and shortening the Allied occupation time.

Supreme courts around the world, namely, the Indian Supreme Court, the United States Supreme Court, the West German Federal Constitutional Court, the French *Conseil constitutionnel*, the Japanese Supreme Court, the Canadian Supreme Court, and, to a more limited extent, the International Court of Justice at the Hague, are the focus of analysis in this book. The underlying purpose is to show how supreme judicial bodies play out the legal-realist drama of judges "making," rather than "finding," law and to expose the "legal myth" (the opposite of the realist approach) in an international context.

The book seems to be divisible into two parts: the first part, consisting of the first four chapters, presents disparate information about the role and selection of chief judges, court organization and structure, appointment by chief executives, terms in office, removal of judges, and measures of descriptive characteristics of justices who serve on these courts. The major problem with the analysis in this beginning section is an insufficient attempt to relate this enormous array of factual information to the theme of the book; that is, to examine how these variables impact on, and are affected by, the role of the selected supreme courts in constitutional review.

Especially disappointing in dealing with this mix of countries is that there is not enough recognition of the differential effects of civil-law and common-law systems on judicial review. The author begins chapter 2 by announcing that he has identified three principles that separate these two types of legal systems, but he does not adequately discuss what would appear to be a major question to legal scholars, namely, whether judicial review appears in different guises because of these two legal traditions. After all, judicial review is based on a principle of judicial supremacy over legislative and executive actions; this is a practice certainly more familiar to common-law states. Surely it is worthy of more notice that such courts as the *Conseil constitutionnel* and the

Federal Constitutional Court, arising out of civil-law traditions, perform roles similar to courts in countries with common-law backgrounds.

The last part of the book, however, dealing with judicial interpretation of constitutional charters, makes it all worthwhile. These chapters offer an excellent discussion of courts in the context of national political and legal arrangements and present comparisons of dimensions of judicial policy-making by identifying types of national legal systems on an activism-restraint policy-making continuum. The major issue of constitutional interpretation, according to McWhinney, is the behavior of supreme courts in adopting a preference for "strict and complete legalism" as opposed to the imposition of judicial value judgments (p. 92). I agree with him that it is necessary to question whether legalism is possible or even desirable in decision making on "the great political and social issues of the day" (p. 92).

McWhinney differentiates primarily between constitutions patterned after a U.S. model, ones with a "lapidarian text, short and succinct" and constitutions with a "detailed social blueprint" (p. 89) commonly found today in Eastern European countries. The discussion from chapter 5 to the end of the book is devoted to explication of the legal-realist premise that judges play active roles in policy-making when they interpret the constitutions of their respective states on such major questions as social policy, federal arrangements, institutional (political) questions, and economic policy. This analysis is especially valuable as a means of comparing the United States with other countries of the world—something that U.S. legal scholars are not often enough given opportunities to do.

McWhinney concludes by observing that he has found that "the same problems tend to recur in all societies, and that the judicially found solutions are often remarkably similar in spite of the seeming disparateness of the constitutional charters involved" (p. 279). All in all, this book plays an important role in bringing us much closer to an understanding of this basic truth.

SUSAN GLUCK MEZEY

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Syria under Assad. Edited by Moshe Ma'oz and Avner Yaniv. (New York: St. Martin's, 1986. 273p. $35.00).

Syrian Intervention in Lebanon. By Naomi Joy Weinberger. (New York: Oxford University Press, 1986. 367p. $29.95).

Contemporary Syria poses a series of paradoxes to outside observers. Close relatives and comrades of the current president, Hafiz al-Asad, monopolize the most powerful offices within the state and party apparatus, yet the government's broadly populist policies have generated "support from large sectors of the Syrian population that have benefited from the regime or that share the Ba'ath concepts" (Ma'oz and Yaniv, *Syria under Assad*, 34). Efforts to expand the role of private enterprise in the country's commerce and industry produced substantial levels of economic growth during the 1970s but at the cost of rising political discontent and increasing indebtedness both to Western financial and trading concerns and to the Arab oil-producing states (pp. 44–65). The al-Asad regime has devoted greater amounts of scarce resources to the country's armed forces over the last decade or so, while allowing social-welfare programs upon which much of its popular support depends to languish (pp. 72–76). Syria's leadership has, since 1970, adopted a consistent policy of reducing the influence of the Ba'th party over such key institutions as the armed forces and state administration but legitimizes its actions in terms of promoting Ba'thi socialism and prohibits rival organizations from playing a substantial role in domestic politics (*Syria under Assad*, 29; *Syria Intervention in Lebanon*, 74–77).

These dynamics have a direct impact on Syria's foreign affairs. Ba'thi policies designed to promote socialism at home continue to prevent Syria and Turkey from establishing diplomatic relations that are anything but "markedly chilly" (*Syria under Assad*, 94–97). Domestic opposition made it impossible for the al-Asad regime to implement programs intended to stabilize Lebanon during the years from 1977 to 1981 (pp. 184–85). Rivalry between the more radical Ba'ath associated with Salah Jadid and pragmatists allied to al-Asad laid the groundwork for Syria's abandoning al-Fath and creating competing Palestinian organizations such as al-Sa'iqah and the

Palestine Liberation Front in the mid-1960s (p. 194). And shifts within the Ba'thi regime largely determined the character of relations between Damascus and Moscow over the last two decades (pp. 227, 230).

But no other event demonstrates the connection between the country's domestic politics and its foreign policy more clearly than the initial phases of Syria's intervention in the Lebanese civil war. As Naomi Weinberger shows, "Syria's military elite" faced growing sectarian and "socioeconomic" discontent in the years after 1973 (pp. 77–79). Whatever stability was present within Syrian society was increasingly a consequence of the regime's use of armed coercion to dominate such challengers as the Muslim Brotherhood (p. 81; cf. *Syria under Assad,* 31–34). Thus, when the situation in Lebanon began to deteriorate in the winter of 1975-76—and persistent disunity within the Arab world precluded effective opposition to Syrian action (*Syrian Intervention in Lebanon,* 265–68)—the al-Asad regime dispatched first the Syrian-sponsored Yarmouk brigade of the Palestine Liberation Army and then regular units of the Syrian armed forces across the border into Lebanon (pp. 180–83, 213–14, 225). In retrospect, it is not clear that this move was worth the cost: Syria was for the most part unable to bring order among Lebanon's warring militias; refugees from the fighting imposed a serious burden upon Syria's already shaky economy (p. 234); and support for the Maronite Christian Kata'ib generated open discontent among Syria's Sunni Muslim community (pp. 236–37). All of these factors prevented the al-Asad regime from achieving its underlying objective in Lebanon—that of restoring some degree of orderliness to Lebanese affairs, if not of recreating the pre-1975 status quo.

Weinberger herself rejects this line of argument, claiming instead that Syria's intervention in Lebanon was related to "multiple miscalculations" on the part of the al-Asad regime (pp. 16, 326–27, 337). These misperceptions were largely predicated upon the regime's vocal support for the Palestinians residing in Lebanon in general and for the mainstream Palestine Liberation Organization in particular (pp. 112–15 and chap. 5). In her view, defending the Palestinians from the Kata'ib and other Lebanese forces, while preventing Yasir Arafat from consolidating a tactical alliance with Kamal Jumblat's Lebanese National Movement, could have provided Syria with a principled basis for claiming a leadership position in inter-Arab affairs (pp. 14–16, 114, 241). This perspective is congruent with those of Yair Hirschfeld, who argues that the alliance between Damascus and postrevolutionary Tehran serves as a way of enhancing Syrian strength and prestige relative to Jordan and the Arab Gulf states (*Syria under Assad,* 117–21), and Amazia Baram, who explains trends in Syrian-Iraqi relations in terms of successive ideological shifts in each country's wing of the Ba'th (pp. 128–38).

In the end, this kind of explanation remains both incomplete and unconvincing since it deals only with what Weinberger calls the "motivations" for Syrian foreign policy rather than with the conditions that enable or prevent a regime from carrying out its preferred programs. Although *Syrian Intervention in Lebanon* provides a generally lucid overview of the opening phases of Syria's involvement in the Lebanese conflict, as well as of the pitfalls into which the al-Asad regime has stumbled in the years after 1976, the volume provides no theoretical explanation for why the regime was able to act on its presumed motivations. (On pp. 196–98, Weinberger suggests that Syria completely reoriented its policy toward the conflict when President al-Asad changed his mind about backing Jumblat's forces.) In the absence of a well-developed explanation for Syrian policy that connects internal dynamics to external actions, the structural perspective adopted by Avner Yaniv in his analysis of Syrian-Israeli interaction during the period from 1948 to 1982 (*Syria under Assad,* 157–78) and by Robert Freedman in his essay on Soviet-Syrian relations in 1982–83 (pp. 224–43), as well as by Weinberger in her discussion of Israeli and Soviet-U.S. responses to the Syrian move into Lebanon (chaps. 10 and 11), appears to be the most promising means in the existing literature of unraveling what Ma'oz and Yaniv call "the Syrian puzzle."

FRED H. LAWSON

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Policy Making in a Three Party System: Committees, Coalitions, and Parliament. By Ian Marsh. (New York: Methuen, 1986. 264p. $65.00).

The rise of the Liberal-Social Democratic Party (SDP) alliance in the early 1980s has fueled vigorous debate among observers of the British political system about the prospect of a "hung parliament," perhaps marking the end of a very long era of two-party adversarial politics. In February 1987 the Greenwich by-election in southeast London resulted in a spectacular success for the SDP at the expense of both the other main political parties. Clearly, as Ian Marsh says in his Introduction, "A House of Commons in which no single party commands a majority of members can no longer be considered fanciful" (p. 1). But how in practice would such a parliament operate? What impact would a no-majority situation have on the way in which the processes of government and policy-making are conducted? Would the discussion of policy become more open, or would powerful political parties, forced into formal coalitions or less formal pacts, be even more inclined than they are now to hammer out their compromises privately in smoke-filled rooms? Could a hung House of Commons break free of the shackles of executive domination and carve out for itself a more significant and positive role in the ordering of policy priorities and the mounting of initiatives? Would the fragile principles of individual and collective ministerial responsibility, and the bruised conventions of civil-service neutrality—not to mention the mechanistic functioning of constitutional monarchy—survive such a change?

Inevitably, much of the discussion on this subject has been highly speculative, and some of the conclusions reached have wildly exaggerated the likely short-term impact of such a result. Hung parliaments could take many different forms. The outturn of events would depend, among other things, not only upon the party political arithmetic but also, crucially, upon how long the situation lasts. After all, the most probable consequence of a hung election result is the prospect of a rerun in a few months' time, as happened in February and October 1974; but the persistence of a hung parliament for a decade or so, and a lasting improvement in the alliance's capacity to translate its votes into seats (buttressed perhaps by the introduction of proportional representation), would almost certainly necessitate a major reappraisal of the way in which British government is conducted.

So far as the House of Commons itself is concerned, it may be tempting to speculate, as Ian Marsh does here, about the potential role of investigative select committees in such circumstances. Westminster's committees inevitably tend to operate on the sidelines of a government-dominated parliamentary system, being concerned with scrutinizing and informing rather than with active decision making, but they have long been seen by optimistic parliamentary reformers as holding the key to redressing a perceived imbalance of power between executive and legislature. The system was significantly changed in 1979 with the establishment of 14 departmentally related select committees empowered to investigate public policy, administration, and finance across the whole range of government responsibility. Might not the new committees, relatively tranquil islands of bipartisan inquiry in a storm-blown ocean of adversarial politics and perhaps possessing the untapped potential to acquire, as Marsh suggests, "independent powers of political initiative," find a special niche in a hung parliament as, "an essential buttress to effective government"? (p. 2).

Marsh's book has two distinct aspects. The first is the product of impressively thorough empirical research. The second is based almost entirely on wishful thinking, only tenuously reinforced by the author's research findings. In the research-based part of the exercise, the author traces the development of the present committee system. He examines with commendable thoroughness the scope and impact of committee inquiries and reports in the fields of economic policy and budgeting, and in relation to "current issues" and strategic policy-making, concentrating on the session of 1982–82. Description of his findings is clear, and there are abundant and helpful summaries in tabular form. His conclusions about the impact of committees are, for the most part, careful and well balanced, though he sometimes exaggerates the significance of committee activity, as when he claims that "there is evidence that committees have crafted a role for themselves in each phase of the budget process" (p. 61): that role, in so far as it exists at all, is episodic and highly marginal.

The author has digested a large amount of paper and talked to a very large number of people involved, in one way or another, in committee activity. There is a separate chapter on select committees and interest groups, based largely on the results of a wide-ranging questionnaire survey, supplemented by interviews. The author concludes that committees themselves have made extensive if patchy use of interest-group evidence but have done little actively to cultivate their "outreach" to such groups; the latter, for their part, remain uncertain about the role of committees but generally welcome their "visibility" and their "fairness." There is, the author believes, considerable undeveloped potential for enhancing the scope of interaction between committees and groups.

So far, so good, but the more general, speculative conclusions of the book require the reader to make an imaginative leap from the real world into make-believe. Marsh's working premise is derived from Samuel Beer's indictment of "pluralist stagnation" (*Britain against Itself* [New York: Norton, 1982]). He argues in his introductory chapter that the "drift and immobilism" of current policy making (he does not give a clear impression of what exactly he has in mind here) could best be remedied by integrating "recalcitrant pressure groups" more effectively and comprehensively into the policy-making process via the select-committee system.

This is a reasonable enough starting point. The trouble is that the author fails to sustain his thesis on the evidence derived from his research and has to fall back on assertion and guesswork. No doubt there *is* unrealized potential for committees to act more extensively as a forum for the articulation and mediation of group demands, but the recent record of committees provides no indication at all of the likelihood of this happening. Arguably, if it did happen, it *might* be a good thing (though one doubts if it would be a panacaea for pluralist stagnation), but this is a matter of both opinion and prediction. And the author himself acknowledges that the views of the Conservative and Labour parties are essentially hostile to such a development. Even the alliance, notwithstanding SDP leader David Owen's general sympathy towards enlarging the role of committees, has not so far begun to go down the road signposted in this book. And whether a hung parliament, of whatever kind, would advance the author's cause takes us even further into the realm of fantasy. It is a pity that the book becomes so wildly speculative, because it contains some interesting things.

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Liverpool on the Brink. By Michael Parkinson (Newbury, England: Policy Journals, 1986. 184p. £22.50 cloth; £9.50 paper).

On 12 March 1987, the Judicial Committee of the House of Lords ruled against 47 Liverpool city councillors and thereby disqualified them from office for five years and left them with total debts of over £500 thousand. This decision marked the end of a four-year fight between the Labour party in Liverpool and Mrs. Thatcher's administration. Michael Parkinson's book tells the story of this fascinating struggle.

The volume provides a detailed account of how the political drama unfolded up to autumn 1985 and, although the book suffers from an absence of references and also lacks an index, Parkinson ably highlights the fiscal, political, organizational, economic, and ideological factors in the potent mix that led inexorably to confrontation with the central government—and to the verge of municipal bankruptcy.

As Parkinson clearly shows, Liverpool Council was *underspending* during the period 1974–83, when no party had a majority and the Liberals held the balance of power. They underfunded services and used reserves to avoid raising the rates; this caused dire consequences after the election of the Thatcher administration, as the artificially low level of expenditure became the basis of financial cuts. "The lost decade" was characterized by confusion and a persistent failure to tackle the city's financial shortfall. The political uncertainty led to bureaucratic inertia. Inefficient council services were not reorganized, and municipal housing was not adequately repaired or replaced.

Within the Liverpool Labour party, where years of patronage and complacency had taken their toll, the old guard was ousted during the 1970s by new-breed activists, many of whom supported the views of the newspaper, the *Militant*. Under their influence and as a result of a shift in union power, municipal socialism became Labour's central concern. Between 1979 and 1984, 40 thousand jobs disappeared and "Liverpool became known as the Bermuda Triangle of British capitalism" (p. 12). By 1985 the rate of unemployment was 27% and in these circumstances the local Labour party was determined to safeguard council jobs. The Conservative government was equally committed to reining in what it saw as profligate local-government spending.

Parkinson argues that a clash between two ideological responses to urban decline was at the heart of the conflict between Liverpool and the Thatcher government. In addition, the national leadership of the Labour party became increasingly embarrassed by the Liverpool councillors' strategy.

Liverpool on the Brink tells the story of how one council challenged the British government and won a round but lost the fight. Parkinson convincingly shows that the local Labour leadership was politically conceited and tactically inept. Nevertheless, although a combination of factors brought Liverpool to the brink, a principal cause was a blundering and obstinate central government. Although the center has control over legislation and financial grants, local authorities have resources of their own, such as the ability to raise finance, control over the implementation of policies, legitimacy arising from local elections, and access to local expertise and information. Consequently, relations between central and local government in Britain have been generally characterized by bargaining and accommodation.

From 1979 onwards, however, the position changed. Mrs. Thatcher's government arrived with scarcely concealed contempt for politics by negotiation and consensus and with a determination to impose its policies on local government. This approach predictably led to considerable opposition. Much of the resistance has been covert. Resentful local authorities manipulated their finances and procrastinated in response to central directives. In Liverpool's case, however, the opposition was stridently defiant.

According to Parkinson (p. 36), the Liverpool political and fiscal crisis was like a Greek tragedy in which the ending was determined as the play opened. But what ending has been reached? Liverpool City Council's debts are currently estimated at £850 million. Unemployment and social deprivation are widespread. Serious public disorder may recur. And the people of Liverpool may once more vote councillors into office who, like their disqualified predecessors, are prepared to defy the British government.

JOHN BENYON

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Israel at the Polls, 1981: A Study of the Knesset Elections. Edited by Howard R. Penniman and Daniel J. Elazar (Bloomington: Indiana University Press, 1986. 280p. $35.00).

For some years the American Enterprise Institute, under the editorship of Howard Penniman, has been sponsoring election studies of selected countries, with each successive election becoming the subject of a volume of collected articles. Of these volumes, the editions on Israel have complemented another series that the political scientist Asher Arian has been editing in Israel since the 1960s. Together, Penniman, Arian, and their contributing authors have given English-language readers excellent access to a political system in which all that can happen in parliamentary politics often does happen.

One drawback of the Penniman-Elazar volume on the 1981 Israeli election is that it was published five years after the event. Major changes have taken place in Israeli politics since then, including the 1983 retirement of Menachem Begin after the Lebanon War of 1982. Events such as these are covered only in a concluding article by Daniel Elazar, but even here the concluding year is 1983. Not mentioned are the 1984 election and the creation of Israel's first government based on a grand coalition of Labor and the Likud.

The value of this book therefore lies less in its description of one election and more in its treatment of long-term political trends. Valuable too are the political-cultural insights into a changing Israel provided by well-informed academic observers.

Among the trends discussed in depth is the consolidation undergone by the Labor and the Likud alliances, or blocs of parties, so that each is becoming a party rather than a bloc. Israel has acquired a quasi two-party system, in which the two major parties are equally capable of contesting for control of the government. The smaller parties have steadily declined as survivable entities. A case in point is the decline of the National Religious Party (NRP) from its former prominence as an important coalition partner and representative of religious opinion.

Changes in the party system are linked to trends within the political culture. There has been a blurring of the distinction between secular and religious politics in Israel as a new "civil religion" emerges blending rabbinical Judaism with the events of Israeli national existence (for more on this, see Charles S. Liebman and Eliezer Don-Yehiya, *Civil Religion in Israel* [Berkeley: University of California Press, 1983]). Generational changes within political elites and among the electorate have been rendering older ideological labels ever less meaningful despite continuation of the ideological style in Israeli political discourse. Events and elections have legitimized some leaders and factions and delegitimized others. The Israeli Arab minority has come into the mainstream of Jewish party politics rather than remain an alienated group of protest voters. Prohibitions on Israeli and U.S. interventions in each others' political systems have declined in effectiveness, and the relationship between the Israeli and U.S. Jewish communities has experienced strains.

Many of the articles in the Penniman-Elazar volume that cover these trends are valuable encapsulations of ongoing research undertaken either by the authors themselves or by others. Torgovnik and Arian, to cite just two of the contributors to this book, summarize their own research pointing to the steady long-term rise in support for the Likud, and the decline of the Labor Party's ideological distinctiveness (see Arian, *Politics in Israel* [Chatham, NJ: Chatham House, 1985]). Labor has become a party that tries to appeal to as many segments of the electorate as possible. But it is unable to secure loyalty among large numbers of younger Israelis unimpressed by the party's heroic past or among many voters from the "Second Israel" (Sephardic and Oriental Jews).

Shmuel Shandler, another contributor to this book, offers a conceptual historical treatment of the religious parties, thus continuing the sociological style of analyzing party behavior pioneered in Israel by Yonathan Shapiro (*The Formative Years of the Israeli Labor Party* [Beverly Hills: Sage, 1976]) and Horowitz and Lissak (*The Origins of the Israeli Polity* [Chicago: University of Chicago Press, 1978]). His eye-opening article delineates the process whereby ideological blurring among many parties has occurred. That process started with Labor's abandonment of its "consociational" relationship with the NRP after Ben Gurion's departure from Mapai.

So long as Mapai avoided mergers with other socialist parties and relied on the NRP for support in coalition making, the NRP elite respected the Mapai's dominance in domestic and foreign affairs. But the new Labor alignment's reduced need for partnership with the NRP set the stage for younger NRP leaders to take positions on formerly secular matters. This they would do in the wake of the 1967 war and the capture of what many Israelis now call "Judea and Samaria." Yet the NRP suffered because the emergence of the Likud bloc gave religious voters a new way to express themselves. Even regular NRP voters felt safe in turning to a Likud that posed no threat to the place of religion in Israeli life. Those same voters were now apprehensive of a secular Labor alignment no longer made cautious on religious issues by a need to preserve its ties with the NRP.

Each article in the Penniman-Elazar book shows that care was taken to place the 1981 election within historical-developmental contexts of this kind. The most impressive articles in terms of identifying linkages of subtlety and elegance are those of Elazar, Brichta, Reich, Shandler, and Arian. Elazar writes from his understanding of Israeli and Jewish culture; and Brichta offers a theoretical framework within which Israeli nomination methods can still be called oligarchical despite major efforts at reform. Arian draws from his immense longitudinal store of survey and voting data to reach conclusions explained with sarcastic wit.

Unusual perspectives on Israeli elections are provided by Samuel Krislov in describing the U.S. role and by Judith Elizur in detailing the role of the Israeli news media.

The same is true of Efraim Torgovnik who writes of the Labor party with the knowledge of an insider and with much delicious detail about who-did-what-to-whom. Indeed, an element that could be even more emphasized in a serious study like this one is the sheer fun of watching the antics of Israeli *askan tziburim* (public persons, i.e., politicians). To see the deal making, the betrayals, the names people call each other, the public reconciliations, the decisions achieved by bluster and brinksmanship, the marriages of high principle and low machine manipulation is to capture much of the true flavor of Israeli political life. I hope more of that flavor appears in any 1984 Israel election volumes issued by these or other editors.

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Government and Politics of Singapore. Edited by Jon S.T. Quah, Chan Heng Chee, and Seah Chee Meow (Singapore: Oxford University Press, 1985. xviii, 324p. $10.85).

This book, completed in 1980, was a project supported by the Southeast Asian Studies Program of the Ford Foundation. The eight authors, including Professors Quah, Chan, and Seah, who served as editors, are Singaporeans teaching at the National University of Singapore. Dr. Quah coordinated the project and also contributed two chapters.

The collection of 12 original essays is designed as a textbook for Singapore University students. It analyzes a country whose prime minister, shortly before independence, warned that an independent Singapore was an economic, geographic, and political absurdity. The two bibliographies, one at the end of the Introduction and one at the end of the book, are well chosen and extensive. Further detailed references are found in the endnotes to each chapter.

The Introduction sympathetically distinguishes the inside and outside researcher and alerts us to the interest of outsiders in Singapore during the 1960s and early 1970s, years of considerable foreign research on Southeast Asis. Unfortunately, an era of cooperative research between insiders and outsiders has yet to emerge. They have data and perspectives that complement each other, and collaboration should augment our knowledge of and perspectives on Singapore and the Third World substantially.

This work is divided into three parts. Part 1 deals with political, economic, and social-cultural conditioners. Part 2, the longest, deals with the contemporary political system. Part 3 analyzes four policy areas—education, public housing, public transportation, and foreign policy. The chapters are not especially critical in their approach, but neither do they avoid pointing out political shortfalls and problems.

Part 1 offers a useful balance between detail and analysis. The British left behind a modern political infrastructure. A specious anticolonialism is not part of Singapore's political culture. Seven years after the book was completed, a key issue, identified in chapter 1, remains high on the political agenda—the effort of the ruling People's Action party (PAP) to find a midpoint "between liberalism and benevolent paternalism." There are inevitable gaps. Operation Cold Storage in February 1963 critically wounded the principal opposition party, the Barisna Sosialis, and facilitated the PAP government's efforts to acquire viability and legitimacy. More time should have been allocated to a discussion of government debt as well as income inequality, both mentioned but not analyzed in chapter 2. What is the Singapore identity? Chapter 3's conclusion that "the levels of national identity in Singapore are very high" may be premature. National identity takes decades to evolve. Singapore is a young country. Still to be researched is how and in what ways it is nurturing popular self-identity. Overall, however, part 1 is a very useful heuristic overview.

Part 2, on contemporary politics, focuses on institutions. This is effective in relatively stable, new political systems where most political behavior funnels through existing institutions. An institutional approach frequently is the best entrée to identify and study the processes of influence and decision making as well as to understand how politics actually works. This is an excellent way to look at functions—because functions are essential in defining and describing an institution. Institutional analysis often makes it possible to focus attention immediately on what is of primary political importance. Part 2 evaluates parliament, the civil service, statutory boards, political parties, and parapolitical institutions such as the People's Association, Citizens Consultative Committees, and Residents Committees. Chapters on interest groups, the executive elites, and the judicial system were not included. The latter would be especially useful now, particularly in view of the wide publicity given to former Member of Parliament (MP) Jeyaretnam's legal problems.

Singapore illustrates one form of cabinet government. The cabinet dominates, and rule by administrative technocrats is unbroken since the mid-1960s. One opposition MP now sits in Parliament. Parliament attracted increasing public attention when two MPs were in Parliament. Now that Mr. Opposition, J. B. Jeyaratnan, who served from 1981 until November 1986, has served one month in jail and is excluded from Parliament for five years, parliamentary sessions will be less spirited. Parliament's policy-input functions will remain generally minimal, as they have since the 1960s. Chapter 4 notes a few issues where the government saw no easy or logical solution, and debate was bolder. A few earlier policies, which were made to seem obvious to Singaporeans and non-Singaporeans, are partially unraveling. This might encourage hard-hitting questions by more backbenchers, especially as the second-generation leaders assume increasing responsibility. The single opposition member has many valid questions to raise, though no structured alternative program has been developed by any of the 15 registered opposition parties. Perhaps, as in the British Parliament, the Singapore Parliament will remain primarily a tester and maker of persons, not of policy. The civil-service chapter provides a good overview. Future research should include recruitment, institutional identity, administrative initiative and policy formulation, and the Management Services Division.

The diminishing role of the People's Action party began in the early 1960s. It is not, though, a cultural holdover and still undertakes relevant functions like short-term electioneering. More pervasive problems likely to increase demands for change—a younger, more educated middle class desiring a more open society and inevitable periods of economic stress—may revive functions of grass-roots recruitment, two-way communication flow, and a renewed role for party mobilizers and communicators as well as technocrats. Tensions continue between the need to marshall and coordinate human resources for purposes of economic development and an orderly society, and a growing public desire for individual expression and political pluralism.

Part 3 focuses on four issue areas of the late 1970s. Public housing is solved for all practical purposes. Public transportation alternatives have been considered, but the mass rapid-transit plan selected is on track. The foreign-policy chapter sets out the relevant foreign-policy patterns as they evolved since 1965 and provides an excellent basis for understanding foreign relations in the 1980s. The education analysis is candid in identifying and summarizing conflicts. This latter issue remains the least amenable to a permanent solution. Not included is a chapter on the economy, perhaps because it was not seen as an issue in 1980 or anticipated for the immediate future—which was wrong on both counts.

This book is a successful collection of essays and provides more data and analysis of a developing political system than can be found respecting many other Third World countries. Asianists as well as those interested in Third World political development look forward to a second edition and hope it will be available in paperback for use in upper-division college courses.

THOMAS J. BELLOWS

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Democracy and Pluralism in Africa. Edited by Dov Ronen (Boulder, CO: Lynne Rienner, 1986. xi, 220p. $26.50).

Probably no scholar of African politics would question the value to participants of a small, intimate seminar on topics such as democracy and pluralism in Africa, especially one sponsored by the United States Information Agency (p. xi) with funds to include a large number of African contributors. The value of the printed proceedings of such a conference is more problematic.

This book does not address the question implicit in its title. It is not a collection of essays on the relationship between pluralism and democracy in Africa. Indeed, there is no consensus among the 18 contributors about what the two terms mean, nor does the editor seem to have seen fit to organize discussion around any particular theoretical starting point, though he himself, in the closing chapter, defines the two terms as sociocultural heterogeneity *(pluralism)* and self-rule and self-determination *(democracy)* (pp. 194, 202). Had these two definitions constituted the theoretical starting point of the book, one might at least take pleasure in reading a serious debate. Instead, one encounters a hodgepodge of definitions and issues, most of them inadequately addressed in articles of 10 pages or less. Pathe Diagne, for example, defines *pluralism* as almost all differences among African societies, including religious, artistic, and ethical differences. Richard Sklar, in an otherwise interesting short—though perhaps prematurely optimistic—piece on the rule of law in Zimbabwe, definies *pluralism* as divided power (p. 141), perhaps more appropriately viewed as a component part of democracy. Pauline H. Baker uses the term *democracy* interchangeably with *political stability* (p. 53). The term *constitutionalism* is used rather weakly by John W. Harbeson, who appears to think that it means merely the presence of a written constitution rather than the actual subjection of government to the rule of law.

Interesting disagreements among the authors go unnoticed. John A. A. Ayoade argues that in Africa, one cannot "opt for . . . aloofness because the wielders of power invade every citizen's privacy" (p. 29), yet Ilunga Kabongo maintains that African political systems are not dictatorships but rather "inefficient polit[ies] where democracy is negatively expressed" and "the peasants always have the exit option" (p. 37). Which, one wonders, is the more accurate picture?

Two pieces address the ideologically obligatory thesis of African "tradition" and how Africa's inherent democracy, which I would prefer to call *gerontocracy*, can or cannot be translated into modern practice. David N. Magang discusses Botswana's successful democratic system but appears not to notice his own point that Botswana is in fact a homogeneous, not a pluralistic, society (p. 106). Lansine Kaba discusses power and democracy in the Songhay empire of the fifteenth to sixteenth centuries, with a brief conclusion that "a fair and long-term effective political system requires autonomous institutions" (p. 100). But the myth of African traditional democracy is not addressed head-on, nor are the real elements of caste, slavery, and gender discrimination in precolonial societies discussed, although Colin Legum briefly notes, in a refreshingly frank overview, that "there are examples of both authoritarian and democratic systems in premodern times" (p. 179).

In general, the participants in the seminar that resulted in this book do not appear to have reconsidered their original contributions in the light of subsequent discussion. While some pieces, such as W. A. E. Skurnik's article on press freedom, stand well alone, the volume as a whole does not cohere. Some articles are on democracy, some are on pluralism, very few discuss the relationship of the two, and no two use the same definitions. This volume is useful as an introduction to the thinking of some prominent African(ist) scholars, but it is not what the title leads the reader to hope for.

RHODA E. HOWARD

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Voters Begin to Choose: From Closed Class to Open Elections. By Richard Rose and Ian McAllister (Beverly Hills: Sage, 1986. vii, 178p. $40.00, cloth; $14.95, paper).

The sense of change and uncertainty in the British party system and electoral behavior is conveyed by the title of this book. The long established two-party, two-class framework accounts for a smaller part of electoral behavior. This important study draws largely on the monthly Gallup surveys and successive British General Election surveys, including the most recent for 1983. The authors argue that the widely accepted model of British voting behavior is one in which voting is *determined* by society, in this case by social class. *Indeterminate* voting is when short-term influences are more important than enduring factors like class and party identification. Party competition can be *minimal* or it can be *multiparty.* The authors combine these characteristics of electors and parties to give four types of electoral competition. Britain has moved from the *closed-class* model, in which two parties compete for a socially determined electorate to *open competition,* that is, an indeterminate electorate and many parties.

Rose and McAllister reject any single-factor explanation of voting, though they make use of class, party identification, and issues to provide an umbrella. Social class has become looser, the manual working class is steadily declining in numbers and in share of the population, and the Labour party is getting a steadily smaller share of a steadily declining constituency. Each social class now splits across the three parties, with the major divisions within the working class. Labour gets just over half of the unskilled working class, and the Conservatives 42% of the skilled working class and 56% of the middle class. Whichever definition of class is used, it shows a consistently weak relationship with voting.

Party identification is also declining in importance. Between 1964 and 1983 the proportion of Conservative identifiers fell from 40% to 36% while Labour identifiers fell from 43% to 31%. The weakening of the traditional moorings of the vote, class and partisanship, has increased the scope for issues. But, in view of the general agreement on many issues across voters of all three parties, Rose and McAllister reject issues as an explanation. An explanation resting upon political principles is also rejected, in view of the lack of clear party divisions on such principles as welfare, traditional morality, and racialism. The authors find little support for the neo-Marxian explanations of cleavages of public versus private consumption and production.

If so many of our traditional approaches are found wanting, what does influence voting? Rose and McAllister provide a "class-plus" explanation. They use factor analysis to set out the contribution different variables make to voting. Not surprisingly they find that each factor makes some contribution, though it is interesting to see the relative importance of political principles. What also emerges as important are class-related or economic-interest factors such as housing and trade-union membership. Within any social class homeowners and council tenants are 20% more likely to vote Conservative or Labour respectively, compared to persons without these attributes. The most solid group of Conservative voters are the nonunion owner occupiers, 62% of whom voted Conservative in 1983. The most solid Labour voters are working-class council house tenants, 61% of whom voted Labour. But the first group is 2½ times bigger than the latter and the ratio has been worsening over time. In 1964 the former outnumbered the latter 34% to 22%; in 1983 they outnumbered them 40% to 17%. The ratio is likely to have widened even more by the time of the next election. In many ways, the alliance is now the more "typical" party in terms of its support. Its vote is minimally influenced by social structure, and on many issues alliance voters' position lies between that of voters for the other parties. Rose and McAllister wonder if we are all alliance today?

Rose and McAllister do well to remind us that class, though declining in importance, never has had the significance in Britain that the number of commentators once attributed to it. For the twentieth century and even since 1945—and the emergence of the Labour party as a majority government for the first time—a substantial part of the working class always voted Conservative. Without that "deviant" minority Britain would not have had a competitive party system and there would not have been Conservative domination. An interesting question is, What is the Labour party, the party of the working class, to do without a majority working class?

It is interesting to compare *Voters Begin To Choose* with two other analyses of the 1983 election. *How Britain Votes* (Oxford: Pergamon, 1985) by Anthony Heath, Roger Jowell, and John Curtice is the successor to the Butler and Stokes landmark study, *Political Change in Britain* (New York: St. Martin's, 1969). It argues that social class, though declining in significance, is still important. But the authors have provided a trichotomous division of the electorate into the middle, intermediate, and working class. Their own approach to social class has resulted in a reduction of so-called manual workers into a "purer" type and, not surprisingly, this type is more inclined to vote Labour than is found with other definitions of *working class*. Even then, however, they find that only 49% of the working class voted Labour in 1949. *British Democracy at the Crossroads* (Boston: Allen & Unwin, 1985) by Patrick Dunleavey and Christopher Husbands is a highly original study that draws on many concepts and insights from U.S. social science. Their own emphasis is on a so-called "radical" approach to electoral behavior in which they stress the importance of consumption cleavages. A welcome feature of *Voters Begin to Choose* is that it is written so clearly, avoids jargon, and also steers clear of tiresome arguments with proponents of so many other approaches.

DENNIS KAVANAGH

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Political Liberalization in Brazil: Dynamics, Dilemmas, and Future Prospects. Edited by Wayne A. Selcher (Boulder, CO: Westview, 1986. x, 272p. $26.50).

After 1974, Brazil's military regime underwent a gradual process of political liberalization. The process began with President Ernesto Geisel's controlled relaxation of authoritarian rule and continued through the transferal of the reins of power by his successor, General João Figueiredo, to the civilian winner of an indirect presidential election in March 1985. *Abertura* (opening) has yet, at this writing, to develop into a fully representative democracy. This prolonged democratization poses a special scholarly challenge. Analysts of liberalization must not only chronicle unfolding events, but they must also place them in a proper theoretical context. This collection of six essays by North American authors succeeds admirably in the former, but less well in the latter, effort. Overall, it makes a valuable contribution to a growing literature.

Three of the essays present detailed accounts of 1977–85 politics, the period in which the regime's highly controlled *distensão* (decompression) strategy gave way to pressures from below and without, resulting in an *abertura* with its own momentum. Enrique Baloyra's essay focuses on the contradiction between the military as a professional institution and as an incumbent regime. Baloyra places inordinate importance on tensions between the presidential advisory group and the military high command as the crucial determinant of both Geisel's and Figueiredo's strategies. This misplaced emphasis on internecine military struggles rather than on the interplay of political, social, and economic factors may stem, in part, from the author's heavy reliance on English-language sources, especially *Latin America Report*, often regarded as a font of unsubstantiated political gossip. Baloyra contributes, nonetheless, a number of astute observations. Most intriguing is his comparison of Mexico and Brazil. Mexico's military leaders created a strongly associated, but distinctly civilian, party apparatus that could engage directly in politics without losing its institutional identity. The Brazilian military administrations were never as successful in using their own party, Partido Democratico Social (PDS), to fight their political battles. By entering the political realm directly, the military thus compromised its own institutional professionalism.

The essays by Wayne Selcher and David Fleischer cover much the same ground as Baloyra. Selcher gives a lucid and well-informed account of the 1979–85 period, assigning more responsibility for Figueiredo's mismanagement to the onslaught of economic recession, heightened internal social and political demands, and increasing international pressure for austerity measures to cope with the foreign debt. *Abertura* was pushed beyond a purely institutional reform to address larger social-reform issues, and Figueiredo was forced to try to balance authoritarian and democratic forces in an economic crisis within a socioeconomically polarized society. Fleischer focuses more narrowly on the role of the congress and political parties. By describing the casuistry utilized by the government to assure PDS domination in successive municipal, state, and federal elections as well as the presidential succession fight, Fleischer provides a uniquely microscopic view of political procedural mechanisms.

Robert Packenham traces the changing nature of political discourse among Brazilian academics and intellectuals from 1964 to 1985, a discourse that shifted from Marxist domination in the repressive 1960s and 1970s to a more pragmatic post*abertura* liberalism. Much of the essay fixes on the transformation of the work of Fernando Henrique Cardoso, a Brazilian sociologist-turned-politician with whom Packenham has long jousted. Packenham's arguments are interesting in their exploration of the somewhat neglected topic of Brazilian intellectual history. To integrate them more completely into this volume, however, the connection between intellectuals and events occurring in the larger polity itself needed to be more clearly established.

Ronald Schneider attempts to predict Brazil's political future by calculating the changing shares of power held by various political actors. These calculations, based on Schneider's 30 years of study of Brazil, are purely subjective. Schneider is to be commended for his effort to measure specifically what most authors assess with broad brush, but his methodology is problematic. Not only are the 29 categories of political actors not mutually exclusive, but one cannot assume that at any given time all actors are fighting over the same issue. The political arena is multidimensional, and political actors may be interested in any number of issues with varying degrees of intensity.

By far the best contribution to this volume is Donald Share and Scott Mainwaring's comparative analysis of political liberalization in Brazil and Spain. The essay argues that both cases embody "transitions through transaction" in which the regime itself initiates the transition, establishes some limits to political changes, remains a significant electoral force during the transition, and negotiates directly with the democratic opposition. Unlike Portugal's transition through regime breakdown (1974) or Peru's transition by extrication (1980), Brazil and Spain both democratized through carefully paced, staggered reforms. The regime excluded selected opposition groups, ruled out both extensive socioeconomic change and punitive measures against former regime members, marginalized extreme groups, and sought to maintain sufficient popular support so that it could compete electorally. The analogy between Spain and Brazil is occasionally overdrawn, but the authors present a well-organized analysis and provide a useful theoretical prism through which to view liberalization.

Overall, this book offers, for the first time in English, a thorough account of political liberalization in Brazil. Several of the essays tread the same ground, however, and one wishes the editor had included an economic or sociological perspective to counter the heavy emphasis on political factors. With the exception of the Share and Mainwaring essay, which makes a more general contribution to theories of comparative politics, the articles are primarily of interest to those who follow contemporary Brazilian politics, both in and out of the scholarly community.

BARBARA NUNBERG

World Bank

Government Violence and Repression: An Agenda for Research. Edited by Michael Stohl and George A. Lopez (New York: Greenwood, 1986. viii, 278p. $35.00).

Political scientists will not understand Third World politics until they can explain governmental violence and repression. Miles Wolpin notes, "In short, an absolute majority of the population in the developing countries survived under regimes characterized by torture, execution, disappearances, and brutal prison treatment of those suspected of opposing the government" (p. 100). How can one talk about interest articulation or aggregation where every opposition voice is smashed and made speechless? Michael Stohl and George Lopez, first in *The State as Terrorist: The Dynamics of Governmental Violence and Repression* (Westport, CT: Greenwood, 1984) and now in the present volume, have brought together an excellent group of writers to begin to fill the lacuna left by Western social scientists' poor perception of what has been going on in the Third World (and also in the First and Second Worlds).

The previous blindness to what should have been obvious to everyone had its reasons, and one was the lack of conceptual tools. Given that the existing conceptual tools failed to take into account governmental violence and repression, how should one start to develop the proper equipment for studying the subject? Some things can be done by just working on the concept in question: defining it, suggesting how to measure it, estimating values and running correlations, postulating hypotheses, and so on, and this volume does a good job here. In the long run, however, we will have to frame a picture of the social and political world that integrates force, coercion, and terror into a wider view of how politics works. While the book makes a few suggestions, most notably in essays by the editors, it still leaves much for future writers.

Several essays use the notion of utility maximization in a decision model, which provides insight but holds dangers and blind spots as well. The insight is that governments are trying to accomplish something when they kill people. The bias is that it makes the decision to terrorize appear too rational, ignoring entirely the very interesting psychological work from Erich Fromm and T. W. Adorno to Milton

Rokeach and now Robert Jay Lifton. It also makes states seem too much as unitary actors, which, if it were the case, would leave no occasion for repression. Hiding in the shadow of this kind of analysis is the metaphor of a polity as an organic body that must rid itself of disease or cancer, such as is exposed in Lopez's discussion of the national-security doctrine. Social utility cannot exist as it is impossible to aggregate individual utilities, and it could not be arrived at if it did exist, given the logical problems of collective action and the paradox of voting. Utility maximization as a rationale for governmental decisions masks their partisan and self-serving character. None of the authors mentions that the most brutal governments are also exceedingly corrupt.

These essays straddle another conceptual problem—the level of analysis. Most of the writing assumes that since the state carries out the violence, the cause of the violence should be at the level of the state or its constituent parts. For instance, Lopez writes about the national-security doctrine in Latin America with no reference to the predominant role of the United States in military training on the continent since 1948. On the other hand, Wolpin in an exhaustive analysis of variance confirms Ruth Sivard's conclusion: "All of the worst offenders are clients of one or the other superpower" (p. 120), and Stohl, in the most interesting conceptual development of the book, points out that much international-relations theory and military strategy portrays the international world in such a way that one should expect intense conflict within countries in the Third World. One of Gurr's 14 hypotheses reflects the same consideration: "Regimes involved in proxy big power conflicts are likely to use the most extreme forms of violence against challengers, including state terrorism" (p. 60).

One has to turn to two other essays to see the correct implication of this tie between international relations and state violence. Grant Wardlaw shows how terrorism breeds counterterrorism if democracies are not alert, which is just what the terrorists want. Robert Friedlander points out that the only real alternative to international terror is that "implausible dream," international law.

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Religion and Modernization in Southeast Asia. By Fred R. von der Mehden (Syracuse, NY: Syracuse University Press, 1986. viii, 240p. $29.95, cloth; $14.95, paper).

In this volume Professor von der Mehden returns to some of the topics discussed in his *Religion and Nationalism in Southeast Asia* (Madison: University of Wisconsin, 1963), but he has broadened the inquiry to grapple with the entire range of modernization issues. He is primarily concerned with Theravada Buddhism in Burma and Thailand, Islam in Malaysia and Indonesia, and Roman Catholicism in the Philippines, although mindful of the situation and role of minorities in all these countries. In the early chapters, the author emphasizes the lack of scholarly consensus on modernization theory, but whatever the conceptual and methodological problems, he regards some notion of modernization as indispensable. The core elements of modernization in his definition are "technological development and the maintenance of a modern nation-state" (p. 48).

In this balanced and judiciously argued study, the author asserts that Western scholars since 1945, like some of their nineteenth-century predecessors, have permitted secularist bias to lead them to the too sweeping conclusion that religion is a massive obstacle to modernization in Southeast Asia. The indictment of religion charges that it instills otherworldliness or fatalism, diverts economic resources from development, supports a nonproductive clergy, stirs up communal conflict, and in general upholds traditionalism and opposes change. While the book contains evidence to give partial support to all of these charges, the author's aim is to show that the "massive obstacle" generalization is seriously flawed. The undifferentiated dismissal of, or attack on, religion obscures too much.

The author is surely on the right track in insisting that the big question be divided into manageable parts. And throughout the book he demonstrates exemplary restraint in providing answers; in many cases the empirical evidence is ambiguous or simply lacking. Methodological problems abound, particularly in trying to link formal religious tenets with the behavior of adherents of that religion. Even when dealing with more concrete contemporary religious institutions—the hierarchy, clergy, and lay organizations—the author is most impressed by the lack of a clear direction. "In sum, although it is difficult to generalize about the role of religious institutions in the process of modernization, recent years have seen expanded efforts toward modernization by both lay organizations and clergy. Technological change remains only a marginal interest to the vast majority of the religious in Southeast Asia, but changes are underway" (p. 98). The reader might wish for a stronger conclusion, but these two sentences accurately summarize the facts presented in the chapter.

The author weighs the pros and cons of religious expenditures as they affect modernization and finds that they are fairly well balanced. The Western critique of popular religious beliefs, he asserts, has frequently missed the point: "At many levels religious belief has been a force in delaying change, but very often as an indistinguishable part of a more complicated process" (p. 201). The author deals with the political manipulation of religious symbols to forge national unity or to foster modernization, and finds that these efforts have frequently been ineffectual and counterproductive. While the influence of religion has been weakened by the forces of modernization, there is no inherent and inevitable conflict, and religion continues to provide identity and security for many educated youth in Southeast Asia.

Von der Mehden has made an important point in this book. The notion of religion as "massive obstacle" has been on the wane for at least a decade and a half but this is the first effort to subject it to systematic analysis with regard to Southeast Asia.

One is left, however, with the sense of an important question not addressed. Surely Buddhism, Islam, and Catholicism are radically different religious systems. One would expect them to have significantly different socioeconomic-political effects. Yet there is little indication in this book that modernization is likely to fare better interacting with one religion than with another, nor is there any systematic analysis of the differing points of tension in the three faiths. The special character of Southeast Asian Islam may have something to do with the disinclination to make comparative statements. With syncretism in Indonesia and communal identity in plural Malaysia, generalizations about Islam do not work nearly as well in Southeast Asia as, say, in Pakistan. Nevertheless, useful comparative statements can be made.

In terms of political modernization, Buddhism, without a divine law to regulate society and with little inclination toward the formation of political ideologies, presents few problems, whereas the notion of man-made laws in a modern legislative state will continue to encounter serious opposition in Muslim societies. Origins sometimes explain little, but it is of continuing relevance that the Buddha was a prince who fled from political power in the pursuit of individual salvation, while the revelation given the Muhammad concerned the building of a total divinely ordered state and society. This is not to suggest that these differences and manifest tendencies are necessarily the most important facts, but they are a part of the overall picture.

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INTERNATIONAL RELATIONS

South Asian Insecurity and the Great Powers. By Barry Buzan and Gowher Rizvi, with Rosemary Foot, Nancy Jetly, B. A. Roberson, and Anita Inder Singh (New York: St. Martin's, 1986. xii, 257 p. $32.50).

We political scientists spend too much time reinventing the wheel. In this case the wheel is called a *security complex* (p. 8). It bears a striking resemblance to groupings of states that have otherwise been called *subordinate systems, subsystems,* or *regional subsystems* since Leonard Binder wrote a pioneering article, "The Middle East as a Subordinate International System" *(World Politics* (NJ) 10 [1958]: 408–29). Subsequently, Michael

Brecher applied a similar approach to the object of study now of concern to Buzan et al. ("International Relations and Asian Studies," *World Politics* (NJ) 15[1963]: 213–35). Taking the perspective a step further, Louis J. Cantori and Steven L. Spiegel published in 1970 a comparative study of subordinate systems, *The International Politics of Regions* (Englewood Cliffs, NJ: Prentice-Hall).

To be fair, Buzan does cite Brecher and Cantori and Spiegel in his introductory chapter ("A Framework for Regional Security Analysis") but implicitly dismisses them as inadequate or at least unworthy of replication. By identifying Cantori and Spiegel with "the perspective of integration" (p. 6), Buzan reveals a lack of knowledge not only of the body of Cantori's and Spiegel's regional-systems work but a lack of appreciation of the distinctiveness of the regional-systems approach from integrationist, transactional, and other approaches to regional study. That Cantori and Spiegel distinguish their approach from the integration perspective could not be more definitively stated than in the title and content of their article, "The Analysis of Regional International Politics: The Integration versus the Empirical Systems Approach" (*International Organization* 27 [Autumn 1973]: 465–94). The regional-systems analyst is concerned with disintegrative as well as integrative variables, with conflict as well as cooperation.

In short, there is a body of regional-systems literature built upon and around the tradition of Binder, Brecher, and Cantori and Spiegel of which Buzan et al. were either unaware or which they chose to ignore in their joint work on South Asia. Concepts from that body of work, much of it security oriented, could have served them well. For example, the Cantori-Spiegel notion of *core (International Politics of Regions,* 20) could have been used with much more flexibility than Buzan's *security complex.* A regional subsystem can have by definition (and in "reality," as Buzan properly requires) more than one core, whereas Buzan's analogous nomenclature for a multicore region is that it is a *complex of complexes* (p. 11) or a *supercomplex* (p. 12). The Middle East, with Gulf, Arab-Israeli, and Maghreb cores or security complexes is an example. Furthermore, the Cantori-Spiegel concept of *intrusive system (International Politics of Regions,* 25) is much more elegant and much less confusing than Buzan's *superregional level* of analysis (p. 15), which, he admits, causes him problems in deciding what to do with China: "A case could be made for including China on the global level. . . . The odd position of China means that some confusion will result no matter which level it is included within" (p. 17). It is not confusing to include China in the intrusive system, as Cantori and Spiegel specifically recommend.

While Buzan's introduction is therefore of little use to the international-relations theorist because it neither builds upon long-established conceptual foundations nor, in my view, offers a better alternative approach to regional-security studies, Buzan and Rizvi do deserve credit for assembling a collection of pieces of considerable interest to South Asia security specialists. The substantive chapters (2–8) are generally up-to-date, well presented, and evocative of fresh insights into the multilevel security relations in which South Asian states are involved.

Furthermore, the final chapter by the two principal authors, "The Future of the South Asian Security Complex," does develop useful standards for judging prospects for future system transformations in the area. Buzan and Rizvi convincingly argue that even a change in external power alignments (though unlikely) "in which the Soviet Union is aligned with Pakistan, and China and the U.S. are aligned with India [following, for example, a flip-flop similar to the one that occurred on the Horn of Africa]. . . . from the perspective of our models . . . would not disturb the continuity of the status quo within South Asia. . . . Nothing could illustrate better than this speculation the impact and importance of the local and regional patterns of security relations in the larger picture of international relations as a whole" (p. 239). Nothing indeed. And that is perhaps the greatest overall virtue of this book. It serves to remind globalists and would-be globalists that by ignoring or downplaying regional dynamics they miss much of the essence of world politics.

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Book Reviews: International Relations

In Whose Interest? International Banking and American Foreign Policy. By Benjamin J. Cohen (New Haven: Yale University Press, 1986, xi, 347p. $19.95).

There has been a substantial resurgence of interest in political economy in recent years. This has been particularly noticeable in the area of international economic relations, where economic issues (initially relegated by political scientists to the realm of low politics) are increasingly recognized as politically salient. At the same time, economists are increasingly willing to acknowledge the political constraints that influence economic policymaking, as well as the important interdependencies among economic, security, foreign-policy, and political considerations.

Benjamin J. Cohen, the William Clayton professor of international economic affairs at the Fletcher School of Law and Diplomacy at Tufts University, has been one of the leading contributors to the "new" field of international political economy. *In Whose Interest?* is of the same high quality as his previous important books, *The Question of Imperialism* (New York: Basic Books, 1973) and *Organizing the World's Money* (New York: Basic Books, 1977). Here he treats the complex and increasingly important nexus of issues involved in international banking and U.S. foreign policy. After a useful and aptly titled introduction, "High Finance/High Politics," Cohen presents a fascinating set of chapters on the historical evolution of international banking and an important series of case studies of U.S.-policy episodes involving the Arab money weapon, the freezing of the Iranian financial assets in the United States during the hostage crisis, the Polish debt question, and the current international debt crisis. In his last chapters, he turns to policy recommendations for dealing with this debt crisis and for improving decision-making processes.

Cohen writes from a sensible, middle-of-the-road perspective. He sees most issues in shades of gray; channels of influence are multidirectional and complex. Banks are neither the all-powerful villains of popular Marxist literature nor the ideal performers of efficient-markets theory. Banks, he argues, have fallen prey to disaster myopia with the consequent overlending that is one, but far from the only, important reason for the current international debt crisis. Government is viewed sympathetically, with officials generally striving to do good, but the efficiency of policy initiatives is often undermined by poor planning or bureaucratic infighting, or both.

Despite the sensibleness and lack of drama in Cohen's perspective and recommendations, the reader is caught up and carried along by the writer's skills. A cynic might belittle his suggestions, such as a better information-gathering and planning system and the creation of a structured dialogue between the government and banks, but as one who spent a number of years in government service, I can attest that these are messages well worth repeating. What could be a list of platitudes in the hands of some become meaningful lessons through Cohen's enlightened use of historical examples.

Can we hope to improve our decision-making processes systematically and significantly? Having seen numerous reforms of the formal economic-policy coordination mechanisms within government undertaken with few noticeable results achieved, I have become skeptical of the role institutional innovation can play in this area. Perhaps more important are efforts to encourage longer-range thinking and sensitivity to the complexity of issues, but even here the role of institutional innovation is limited. In the 1970s I directed Treasury's international research and planning staff, which was created for just such purposes. It was actively used by one secretary, largely ignored by a second, and abolished by a third. Thus I am inclined to feel that what is most important is the attitude of senior officials toward the need for good policy analysis. Making Cohen's book mandatory reading for all policy officials would be a good beginning toward improving our policy-making process.

THOMAS D. WILLETT

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Foreign Aid, War, and Economic Development: South Vietnam, 1955–1975. By Douglas C. Dacy (Cambridge: Cambridge University Press, 1986. xix, 300p. $39.50).

Many political scientists see the Vietnam War as the beginning of the end of the United

States' primacy, as the first symptom of its later spastic efforts to regain hegemony by relying on a foreign policy designed almost exclusively around the use of force. But it was more than that (if indeed that is what it was). It was also a test of theories and hopes about the potential of foreign aid as an instrument for the promotion of political and economic development in friendly countries, as an indirect contribution toward the creation of an unthreatening and stable world order. Most books about Vietnam center about the first set of issues; Dacy's book is a thorough and convincing exploration of the second.

There are special circumstances in the Vietnam experience that have discouraged economists and political scientists from drawing generalized conclusions about development. The huge overlay of U.S. foreign aid dominating the economy, the crescendo of rising guerrilla activity culminating in outright war, the hesitant leadership facing apparently insatiable demands for reform, and the absence of the aura of legitimacy, all so overwhelmed the processes of development that the lessons are hard to see. But other nations have faced similar problems, in various combinations. Windfalls like mineral discovery or sudden rise in oil prices, ethnic and ideological unrest, inexperienced governments, and unhistorical claims to power are commonly found equivalents to the special circumstances of Vietnam. Dacy shows how the Vietnamese effort is informative both about the processes of development and the potential application of foreign aid on their behalf.

This carefully documented economic history makes it clear that Vietnamese economic progress during the early years of independence (1955-59) was better than generally reported, though far from sufficient, and that it was political failure that robbed the population of what fruits it produced. The best that foreign aid could do as military costs rose was to check inflation and to encourage an occasional economic reform in the direction of liberalization. One of the by-products of foreign aid—the black market—contributed to both outcomes. A surprising finding is that the major spurt of economic development seemed to have occurred in the wake of the unsuccessful Tet offensive, whose consequences stimulated and liberalized both the economy and polity for a time until the withdrawal of U.S. troops and the full-scale invasion from the North took their toll.

Economic-development experience in the context of international and domestic political uncertainty is a neglected area of empirical research. It is fortunate that it has been so well carried out here. Major economic decisions, such as the tactics of devaluation during a period of inflation, tax measures designed for redistributive purposes in the context of rapid financial turnovers, the fixing of interest rates to encourage savings, the use of parallel foreign-exchange structures for different imports, the consequences of commodity subsidies, and even the consequences of land reform, are all carefully explored. There is an ingenious use of data to estimate such features of the Vietnam experience as comparisons of growth rates across sectors, the effects of the Commercial Import Program on industrial development, the economics of increased "dependency" associated with different aid levels, the monetization of the economy as an indicator of development, changes in farmers' income associated with land reform, and contrasts between rural and urban income changes resulting from different policies. The data expose some absurdities in economic convention, such as the interpretation of foreign aid as "savings" and the theory that control over foreign-exchange levels could offset the inflationary effect of the very presence of U.S. troops. Perhaps the last persuasive argument appears in the final chapter of the book, in which Dacy tries to show how a military threat can contribute to development if a country is receiving foreign aid. This demonstration consists of a pairing of Israel, Taiwan, Korea, and Vietnam with Latin American countries that were "nonthreatened" and "moderately aided," to show that Vietnam was the odd man out in not developing more impressively. But the conclusion, that the relative failure came about because neither the government nor the donor really placed development as a major priority, is fully justified by other data presented in the book.

The reader will find much to learn from the Vietnamese economic experience, and though the achievements of the foreign-aid program are fully documented here, no one will be likely to conclude that the U.S. presence in the later years of independence was a manifestation of superpower, or even great-power, wisdom. This is not a revisionist tract, only a reexamination of an experience from which U.S. citizens have learned all too little.

On the dust jacket, the publisher expresses the modest hope that this book could be used as supplementary material for courses on economic development at both undergraduate and graduate levels. That is a reasonable hope. Courses that make use of this rich case study will be the better for it, as indeed will foreign-policy makers.

JOHN D. MONTGOMERY

Harvard University

Perceived Images: U.S. and Soviet Assumptions and Perceptions in Disarmament. By Daniel Frei (Totowa, NJ: Rowman & Allanheld, 1986. xvii, 323p. $26.50).

Daniel Frei correctly asserts that we need a careful, document-based description of Soviet and U.S. perceptions of one another and of the conceptions on which their approaches to security are based. This volume does not even begin to satisfy this need. It is little more than a string of quotations from official documents cited out of context to present the erroneous impression that both superpowers are monolithic in their respective points of view.

Frei bases his "analysis" on official documents by arguing—really proclaiming—that these are indicative of what policymakers in Moscow and Washington really believe. A careful scholar who chose his documents wisely and interpreted them in context might make a good case that they offered insight into policymakers' minds on a carefully defined range of issues. Frei is utterly insensitive to any of these methodological concerns. Take the documents he uses. For the United States, these are the annual reports of the secretary of defense, U.S. military-posture statements, and press releases from the State Department's Bureau of Public Affairs. These are the *least* likely sources to reveal thinking of officials. They are self-conscious public-relations gestures designed to sell military appropriations to the Congress or justify specific administration foreign policies to elite opinion in this and other countries. Any correspondence between documents of this kind and what policymakers really believe is purely coincidental. Their use is doubly inexcusable in an open society like the United States, where much better documents are available for analysis.

Documents must be interpreted within their proper context. Pronouncements, especially public ones, are always designed to influence particular audiences. Very little can be inferred from such a statement unless one knows who made it, the audience it was aimed at, and the political consideration at the time, internal and external, that influenced its substance, tone, and timing. Students of Soviet and U.S. foreign policy routinely engage in this kind of analysis. Because public documents are all that is generally available in analyzing Soviet policies and decision, Soviet scholars have been forced to develop particularly sophisticated methods for making inferences from their data. Frei makes no attempt to do this or to build on the work of others who have. Instead, he treats all statements as equal in value and equally indicative of what their authors believe. This is either naive or irresponsible.

Finally, there is the question of consensus. Frei portrays both superpowers as unitary in their thinking. This stands in sharp contradiction to the position of numerous highly regarded studies of Soviet and U.S. strategic and foreign policy. They indicate that strategic policies, the view each superpower has of the other, and their respective attitudes toward nuclear war and arms control have evolved considerably over the years. These studies also indicate that there are often substantial areas of disagreement and controversy within the Soviet and U.S. governments at any particular time. The best and most useful analyses are those that document these controversies and use them to shed light both on the substance of policy and the process by which it is made in the two political systems.

Perceived Images is a sham. It pretends to be something that it clearly is not. Surely, the United Nations Institute for Disarmament, which sponsored the study, ought to be able to do better than this.

RICHARD NED LEBOW

Cornell University

Development Strategies Reconsidered. Edited by John P. Lewis and Valeriana Kallab (New Brunswick, NJ: Transaction Books, 1986. viii, 190p. $19.95, cloth; $12.95, paper).

The conventional wisdom in the major international development agencies today is that government is largely responsible for the Third World's disappointing economic performance and that markets are the remedy for most of these problems. Even within the mainstream, however, there remains a healthy degree of skepticism about these neoclassical views, so the Overseas Development Council (ODC) in 1985 brought together several experts to review the current state of development strategy. Eight papers emerging from that conference are reproduced here, the fifth volume of ODC's series on United States–Third World policy perspectives. While none denies the importance of markets, each points out limitations in the laissez-faire paradigm for promoting development and reminds us that governmental and political factors are also critical.

Coeditor John Lewis begins the collection with an overview of the ongoing debate. His spirited defense of foreign aid, which will be familiar to those acquainted with Lewis's work, is guardedly optimistic about planned promotion of economic growth and social equity. In the next essay, Irma Adelman focuses explicitly on the equity issue, and calls for specific programs to expand access to land and education as the first step in development. Equally important, in her view, Third World states need to adopt economic policies that have a high labor content, which means basing development on agriculture or, alternatively, on exports.

John Mellor picks up the agricultural theme in the next paper, renewing his classic argument about how the rural sector can support an employment-based strategy of growth. Government has a major contribution to make by providing public goods such as research, education, and infrastructure. Interestingly, Mellor sees mutual enrichment, not conflict, between an agriculturally oriented strategy and one led by exports.

A noted proponent of export-led development, Jagdish Bhagwati, offers the next paper. Of all the contributors, Bhagwati is most sympathetic to the neoclassical orthodoxy, though he sees virtue in public-private partnerships because they can help assure a stable policy environment. The thrust of his paper, however, is to refute arguments that export-oriented strategies have become less feasible under current world market conditions. Most telling is his observation that outward-looking countries have tended to adjust better than inward-looking ones to the economic shocks of the 1980s, a point reiterated in Leopoldo Solis and Aurelio Montemayor's essay. Colin Bradford, however, warns in his paper against simplistically applying the export-led model, which he shows to be based partly on a misreading of East Asia's experience. He points out that in countries such as Taiwan and South Korea, government intervention and import substitution actually have contributed to the industrialization drive.

In the next essay, Alex Duncan turns to the issue of how donors can help the poorer countries adapt more readily to changes in the global environment, which he feels necessitates greater national capacity for policy analysis and project implementation. According to him, economic assistance should be devoted more directly to improving management and institutions, to enhancing human resources, and to increasing the supply of technology.

The final contribution to this collection is by Atul Kohli, the sole political scientist represented. He disputes the prevailing opinion that authoritarian government is a necessary condition for economic development, pointing to the favorable performance of several democratic developing countries. Kohli is pessimistic about the spread of democracy in the short run, however, the major barrier, in his view, being the fragmentation and competition of Third World elites and not, as the neoclassical paradigm often postulates, the mobilization of the masses.

All the essays collected here are cogent, lively, and informative. Nonspecialists, for whom this volume is particularly intended, will find it a useful introduction to the current debate about development strategy.

ARTHUR A. GOLDSMITH

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Book Reviews: International Relations

Essays in International Law in Honour of Judge Manfred Lachs: Études de Droit International en L'Honneur de Juge Manfred Lachs. Edited by Jerzy Makarczyk (The Hague: Martinus Nijohoff, 1984, 737p. $108.00).

Manfred Lachs has had a 40-year career in international law, as diplomat, scholar, teacher, and, most recently, judge on the International Court of Justice (ICJ). His interests include disarmament (as architect of the Nuclear Free Zone in Central Europe in 1958); peaceful use of outer space (as chairperson of the UN Legal Subcommittee); legal control over local government; and international dispute settlement under the ICJ. The 50 essays in this volume (22 of them in French) reflect this diversity of both subject matter and authors' backgrounds (writers, scholars, practitioners, judges, and jurists from North America, Mexico, France, Great Britain, Poland, Finland, and the USSR, to mention a few).

Several articles should be of special interest to U.S. professors of international law. Jennings ("Teachings and Teaching in International Law"), following Lachs's footsteps, emphasizes that international law cannot be abstracted from diplomatic history or international political-economic events (p. 129). Similarly, McWhinney ("The Time Dimension in International Law, Historical Relativism and Intertemporal Law") discusses the conflict between the old and new in legal doctrine, the status quo versus the "newer political forces oriented to changing law in accord with changing society" (p. 180), and the ICJ decisions that have embraced Intertemporal Law, including *Namibia Advisory Opinion* (1971) and *Western Sahara Advisory Opinion* (1975). For ideological contrast, students might consult Tunkin of Moscow University ("Contemporary International Law—A New Historical Type of International Law") for a thorough but unoriginal exposition of historical types of international law, from slave-owning societies, to feudal and bourgeois law, to contemporary law defined in terms of the transition from capitalist to communist law.

In other articles, the specific role of the ICJ and international organizations is explored. For example, Elias ("How the International Court of Justice Deals with Requests for Advisory Opinions") dissects the relevant legal and political criteria for the organs requesting advisory opinions, with special attention to the *Reparations* case. On a more explicitly political level, Franck ("Finding a Voice: How the Secretary General Makes Himself Heard in the Councils of the Nations") explores cases of the secretary's intervention and refusal and the legal precedent and procedures for such choices.

Finally, two substantive issues of most concern to Lachs are evaluated. Castaneda ("Negotiations on the Exclusive Economic Zone at the Third United Nations Conference on the Law of the Sea") offers a fascinating inside account of the negotiating process. And Blix ("Arms Control Treaties Aimed at Reducing the Military Impact on the Environment") emphasizes the Environmental Modification Convention, 1977, discussing the applicability of various provisions.

Numerous articles are of sufficient merit to the international-law community that library purchase is encouraged. Unfortunately, there is little concerted effort to integrate the material by broad themes. The Prott article ("The Judicial Philosophy of Manfred Lachs") might have provided some of the needed integration, especially had it been placed at the beginning or the end. Readers of books containing such diverse articles deserve to have the basic themes elucidated and my reading of the book suggests that this would not have been an unsurmountable task for the editor.

KAREN MINGST

University of Kentucky

Nuclear Ethics. By Joseph S. Nye, Jr. (New York: Free Press, 1986. xiii, 162p. $14.95).

In this small book the author wrestles thoughtfully with one of the major moral problems of our age, maybe *the* moral problem of our age: is nuclear deterrence proper?

He is uncomfortable with the "self-indulgent" moral absolutists who see nuclear deterrence as unquestionably wicked and so move quickly to some sort of unilateralist or abolitionist position with, for Nye, too little concern for the dangers or practical realities. But he is also unpersuaded by the too cynical exponents of an amoral *realpolitik*, those "realist" strategists who want to minimize moral concerns in foreign-policy making and so tend to sacrifice at the outset the Western values they claim they have adopted nuclear deterrence to protect.

Nye urges a middle course. Policy should be influenced by moral principles, policymakers by moral philosophers. The teachings of both the Kantians, with their rule-oriented perspectives, and the utilitarians or consequentialists, with their act-oriented perspectives, should be consulted. Leaders, strategists, and citizens should weigh all three essential dimensions of sound moral reasoning: ends (or motives), means, and consequences (likely results). Nye's recipe and the maxims he derives are solidly in the "just-war" tradition.

The only motive, or *end*, that can warrant possessing nuclear weapons is the "just cause" of self-defense via nuclear deterrence. But self-defense is given a pretty elastic definition; Washington is entitled, Nye argues, to use nuclear threats to defend not only the mere physical survival of the United States and its citizens but also its allies and friends, its "culture" and democratic institutions, and the current global geopolitical equilibrium (pp. 42–49). Such a broad definition of *self-defense* might license nuclear threats to protect either those far-flung "vital interests" so often invoked or even the overseas commercial investments and operations so important to the affluent U.S. lifestyle. Moreover, with such a definition, Nye or a follower of his maxims might even approve nuclear threats with compellant rather than deterrent intentions, threats designed to compel or persuade Moscow to *do* something (exit Africa, say) rather than to deter Moscow *from* aggression.

The key criteria to help people decide about the propriety of *means* in this arena are the criteria of proportionality and discrimination. Nye argues that nuclear deterrence can meet these tests. Nuclear weapons could conceivably be used in some limited ways such that their use would not be judged as disproportionate by many, though Nye is probably too sanguine about the possibility of controlling escalation up the ladder to clearly disproportionate levels. And if we concede that in the twentieth century the discrimination (or noncombatant immunity) test has already been relaxed to require only the minimization, not the complete avoidance, of civilian casualties, then some usages of nuclear weapons (at sea or against remote military bases) could comply with the discrimination criterion almost as well as many standard (and apparently widely acceptable) usages of highly lethal conventional weaponry.

Consideration of the *consequences* of reliance on nuclear deterrence involves speculations about whether it really deserves the credit it usually gets for having prevented Soviet aggression that would otherwise by now have occurred, more speculations about risks and possibilities for the future, and then yet more speculations about possible psychic and political and economic effects of reliance on nuclear deterrence. Nye points up the hazards of all this speculating. But given his earlier insistence that we "start with a strong presumption in favor of rules and place a substantial burden of proof upon those who wish to turn too quickly to consequentialist arguments" (p. 23), one is left to wonder if he would really accept all these mushy speculations as "proof" tending to support reliance upon nuclear deterrence on consequentialist grounds.

Nye concludes with the usual "owlish" arms controller's suggestions for achieving "balanced deterrence" in order to reduce the risks of nuclear war during the next decade or two or three. And he urges the standard political and diplomatic approaches for improving U.S.-USSR relations in order to enable a gradual reduction of our reliance on the primitive and still morally troubling instrument of threatening catastrophe.

JAMES A. STEGENGA

Purdue University

The New Order of the Oceans. Edited by Giulio Pontecorvo (New York: Columbia University Press, 1986. 277p. $30.00).

Covering some 70 percent of Earth's surface, the world's ocean areas are of significance in terms of food, energy, and mineral resources, as a site for waste disposal, a medium for the transit of the vast bulk of international trade, and as an arena for military operations. In recognition of the importance of ocean areas, three major multilateral diplomatic conferences under the auspices of the United Nations have been held since 1958 to determine the future legal and political order of ocean space. In light of changing technological and political circumstances, the Third United Nations Conference on the Law of the Sea (UNCLOS III) met for a decade and finally adopted a new comprehensive Law of the Sea Convention in 1982. Treating a vast array of ocean rights and duties, this treaty is of substantial complexity and embodies a mixture of both existing and new rules of international ocean law. From the perspectives of allocation of ocean rights and duties and the development of the international legal system, UNCLOS III ranks as one of the major events since World War II. In terms of broader aspects of international relations, it marks perhaps the first major diplomatic conference at which Third World states played a full and independent role in the shaping of a significant part of the international legal system.

The New Order of the Oceans, edited by Professor Giulio Pontecorvo of Columbia University, provides a selection of essays by a distinguished set of experts and diplomats on the resources of the ocean, ocean science, the development of the new ocean regime from the perspectives of Africa, Latin America, Southeast Asia, and the United States, and a speculative essay on the unfinished business of UNCLOS III. Though little of the substantive material in this book is new to specialists in the field, this volume does provide a good review of the major issues and problem areas treated at UNCLOS III. All of the essays are well written and informative; some are outstanding in their insights.

The issues surrounding deep-seabed mining ultimately led the United States government to refuse to sign the 1982 Law of the Sea Treaty. Accordingly, it is fascinating to read the essay by Marne Dubs, former industry executive and expert in ocean mining, who shatters a variety of myths associated with such mining, myths which contributed to rigid negotiating positions on the part of both the United States and a number of developing states. Surveying the current situation, he finds that the United States lead in ocean mining has withered away and that, diplomatically, the United States has been outmaneuvered by Japan and France.

Professor Thomas Clingan of the University of Miami Law School and vice-chairman of the United States delegation to UNCLOS III, provides a thoughtful and revealing assessment of U.S. policy. Even in the early days of the conference, U.S. policy was driven not simply by navigational issues but also by concern with the resources of the continental shelf and the exclusive economic zone. Issues relating to deep-seabed mining were not seen to be of major economic or strategic importance and, Clingan asserts, were manipulated skillfully to draw attention away from matters perceived as having more fundamental significance such as those concerning navigation rights, continental shelf, and the exclusive economic zone. With the advent of the Reagan administration, seabed mining took on a new importance, however. Ambassador Djalal of Indonesia, in his contribution to this volume, sees this change as exemplifying this Reagan administration's "insatiable appetite" for having things its way at UNCLOS III.

In the closing essay of this book, Professor Jonathan Charney of Vanderbilt University Law School examines the important theme that a considerable amount of unfinished business remains in the wake of UNCLOS III and that the evolution of the law of the sea is a continuing process. While the drama attendant on UNCLOS III may be lacking in the postconference world, important developments are ongoing. The law of the sea remains a significant issue in contemporary international relations and *The New Order of the Oceans* contributes to our understanding of ocean law and politics.

LAWRENCE JUDA

University of Rhode Island

U.S. Trade Policies in a Changing World Economy. Edited by Robert M. Stern (Cambridge: MIT Press, 1987. 437p. $25.00).

This book is a nonmathematical survey of the contemporary trade theory and a sample of the efforts of some economists to explicate the politics of trade policy in the United States. The survey of trade theory (notably Deardorff and Stern's essay) will be valuable to political scientists seeking a general knowledge of this area of economics, particularly since several of the chapters deal also with the latest area of innovation in trade theory—the application of the theory of industrial organization and market structures to international trade. This latter development (known as the "strategic-industry" approach to trade theory and policy) is of special importance to political scientists because it makes relevant the behavior of firms and industries in imperfectly competitive markets, where each actor must predict the actions of others. Traditional trade theory assumed perfect competition, narrowing the scope for domestic politics to the very broad income-distribution effects predicted by the Stolper-Samuelson theorem (as in the Magee and Young essay).

The economists' explanations of political behavior are less satisfactory since, with one exception (the chapter by Magee and Young), they succumb to the temptation to treat political factors as exogenous constraints rather than objects of explanation. Magee and Young note that the pure theory of trade would predict that Democrats (representing labor-intensive, import-competing interests) should be more protectionist than Republicans and suggest that the reason this is not always so is that Democrats reduce protectionist pressure with policies that increase inflation and reduce unemployment. The authors of other chapters simply take political objectives as given and ask whether trade barriers are the most efficient method of attaining these goals. Thus, the book not only provides a good background in trade theory but is also useful in pointing out to political scientists areas of trade-policy analysis where they may have some comparative advantage over the economists (e.g., in explaining why governments may choose to build up defense industries with trade barriers rather than subsidies).

In explaining U.S. trade policy, the essays span at least four levels of analysis. Corden, Jackson, and Cooper all focus on the role of, and need for, international institutional constraints on sovereignty. Jackson laments the erosion of the General Agreement on Tariffs and Trade (GATT), which he blames partly on confusion (people do not understand that the terms *most-favored nation, nondiscrimination,* and *GATT* are not coterminous) and recommends the strengthening of both the procedures and structures of GATT. Corden's analysis is more abstract and discusses the need for a global social compact that will meet both efficiency and distributional goals, protect people from the folly of their governments, and promote general harmony.

Several of the essays emphasize the state as a rational actor in the international economy. Srinivasan discusses the integration of defense objectives into trade policy, mostly by attempting to specify the domestic welfare trade-offs in linking the two. He also suggests that the use of trade barriers for military goals is usually unsuccessful, and that trade promotes peaceful political relations. Other essays adopt the rational-actor approach in recommending bargaining tactics (e.g., Dixit's emphasis on the need for the United States to be committed to a tit-for-tat policy of retaliation—though he believes the United States is not institutionally equipped to pursue such a tactic).

Krugman's essay on strategic-industry policy is also in the rational-actor mode, since it assumes a goal of national income maximization and leads to an emphasis on a state's making credible international commitments to increase its own oligopolists' share of global profits. However, the strategic-industry approach is so sensitive to critical assumptions about, for example, numbers of domestic firms, foreign firms' reaction functions, and the internationalization of capital markets, that Krugman concludes there is unlikely to be any gain from attempting to implement a strategic-industry trade policy. Nonetheless, the industrial-organization approach to trade does suggest the importance of domestic interests in explaining trade policy. Dornbusch and Frankel underscore the importance of this aspect of trade policy in an essay concluding that exchange-rate movements have had much less effect on trade policy than have structural changes in industrial organization (e.g., productivity, sectoral interdependencies). None of the essays make use of the large literature on rent-seeking models of interest-group behavior to explain the cross-sectional structure of protection.

All of the papers are worthwhile reading. If there is any general fault, it is that the book exemplifies a typical conundrum for economists: the difficulty of maintaining the abstract rigor of trade theory and at the same time explaining the political realities of trade policy. Political scientists ought to be able to help here, and this book will give them numerous insights into areas where such help may be usefully rendered.

JOHN A. C. CONYBEARE

University of Iowa

International Trade and the Tokyo Round Negotiation. Gilbert R. Winham (Princeton: Princeton University Press, 1986. xi, 449p. $45.00, cloth; $13.50, paper).

This is an important book on an important subject. The past 10 years have seen a flood of theories that purport to explain the behavior of governments in issues of foreign economic policy, theories based on the simple calculus of organized group interests, on the role of the *hegemon*, and on the conditioning power of economic culture. It is refreshing to look closely at the complex processes of a major economic negotiation, constantly looking for evidence of the utility of the various theories.

Predictably, nothing seems to work in quite the way the theories project. Those who prefer to think that national positions in international economic negotiations are the simple sum of organized group interests may take comfort in the fact that pressure groups indeed played a substantial role in the Tokyo Round. But both the context and the weight of that role varied from one country to the next, depending in considerable part on the nature of the processes available to pressure groups to express themselves. In some cases, the pressure-group representatives came to identify themselves with the larger purposes of the negotiation; in others, they accepted their lack of influence with resignation; in still others, they reacted with intransigence.

Consistent with the approach of most negotiation theorists, Winham looks on international negotiations such as the Tokyo Round as representing for each participating country an exercise in two simultaneous negotiations, one an internal negotiation within the country's domestic establishment, the other an external negotiation with other countries. A highlight of his book—a unique effort, as far as I am aware, in the literature of international economic negotiation—is his contrast of the internal negotiating structures of the United States, the European Community, Japan, and Canada. The U.S. structure drew primarily on representatives from the special-interest groups, the EC structure primarily on officials representing national viewpoints, the Japanese structure primarily on representatives of the various government bureaucracies, the Canadian structure primarily on provincial and bureaucratic representatives. Thinking of the nation-state simply as a rational unitary actor or as a faithful representative of its organized interests proves not very helpful in predicting its behavior.

Winham produces a provocative generalization to explain the U.S. Congress's overwhelming support of the Tokyo Round, a generalization worth the price of the book. The 1974 Trade Act, which provided the statutory basis for U.S. participation in the Tokyo Round, contained two innovations of profound importance. First, the act modified the basic constitutional relationship of the Congress to the executive branch in trade matters by assuring the executive in advance that the Congress would consider the negotiation's results on a "fast track," that is, under a special set of rules that guaranteed a simple up-or-down decision within 60 days. Second, the act required the executive to involve the representatives of the organized pressure groups deeply in the negotiation, thereby extracting the Congress itself from its uncomfortable position as middleman between the special interests and the executive.

I shall resist the usual temptation of the reviewer to introduce his own hypotheses in explanation of the success of the Tokyo Round; it is sufficient to note that Winham's view may not prove to be the last word. But his explanation is seminal for anyone concerned with understanding foreign economic policy-making in the United States.

In the end, the book leaves unanswered what may be the most important question of all. If in 1979 governments were able to achieve the remarkable agreement represented by the Tokyo Round, why did the agreement prove so empty in its subsequent impact? Why, indeed, did international trade relations run swiftly downhill in the years immediately following? In his first and final chapters, Winham deals somberly with the disappointing years following the Tokyo Round and the disconcerting prospects for the future. But his ruminations on that subject leave the critical question unanswered.

Still, Winham's careful research has carried us a step nearer to understanding the processes involved in international economic policy-making. His work is especially important in putting some naive theories of economic policy-making in a more realistic perspective.

RAYMOND VERNON

Harvard University

The Dynamics of Deterrence. Frank Zagare (Chicago: University of Chicago Press, 1987. xiii, 194p. $22.00).

For some time now the theory of nuclear deterrence has stood on shaky foundations. Political philosophers, and strategic theorists, as well as U.S. and French clerics have recognized the paradoxical nature of the deterrent threat in the nuclear age: the seeming irrationality of response once deterrence fails, and the dilemma of disassociating the threat from the use of nuclear weapons. Given such contradictions, Frank Zagare observes, "If deterrence were a building, it would probably be condemned" (p. 1). His goal in this book, therefore, is to provide a sturdier framework on which to place the strategic doctrines of the superpowers. Unfortunately, the author erects only a flimsy frame of revealed preferences.

Zagare argues that deterrence works when it has the characteristics of a prisoner's dilemma (PD) game, where the actors prefer war to unilateral disadvantage (the action that is being deterred). Such a formulation is immediately counterintuitive because the Nash equilibrium of the classic PD game is mutual defection or, in this case, war. If deterrence really resembles such a game, then one should *expect* deterrence to fail and, in a nuclear world, mutual destruction to result. Zagare argues that the inadequacy of this formulation lies not with seeing deterrence as a PD game but rather results from the limitations of classic game theory where actors choose strategies *simultaneously*. By simply stipulating that actors can move *sequentially* and in response to an opponent's strategy, mutual cooperation actually emerges as a "non-myopic equilibrium" in a PD game. This can be seen by following the reasoning of players who start from mutual cooperation in prisoner's dilemma: if one defects to get his or her best outcome, *then* the opponent will also defect leading to mutual combat, which is a worse outcome than the initial status quo. Zagare's "theory of moves" presumes that both actors have the foresight to recognize that unilateral defection will lead to a worse outcome than one's starting position.

The remaining problem for a theory of deterrence in the nuclear age lies in whether or not the leaders of the United States and the Soviet Union really prefer mutual destruction to some form of capitulation. The author argues that the revealed preferences of the superpowers support such an interpretation; historical examples such as Masada and the Alamo are evidence that actors often reveal a preference for some value above life itself; moreover, such preferences are rational. As Zagare states, "Statesmen seem to prefer, or *at least are perceived to prefer,* [in accord with the slogan] 'better dead than red' or alternatively, 'better a grave than a capitalist wage slave'" (p. 174; my emphasis).

Herein lie two fundamental problems with *The Dynamics of Deterrence.* First, for rationality to be useful as a concept at all, we must be able to stipulate its limits and to recognize that some actions are irrational. Usually we do so by relating the efficacy of an action to the goal that one is trying to attain. In the cases of the Texans at the Alamo and the Jews at Masada, martyrdom is rational when it keeps a larger dream alive, or when losing a battle can lead to winning a war. Likewise, fighting on is rational if combatants believe that they will be killed when captured. Such martyrdom should be seen as irrational, however, as a counterthreat carried out when deterrence has failed. I am willing to grant rationality to the slaughtered at Masada and the Alamo on the principle of charity but not on the basis of them carrying out a suicidal threat when deterrence failed. There were not battles undertaken as a counterresponse when a preferred status quo was upset but battles in a larger war to upset a status quo predicated on the domination of some of the participants. These examples are entirely irrelevant to a discussion of nuclear deterrence and fail to resolve the paradox of counterresponse as irrational in a nuclear age.

Second, while nonnuclear deterrence may resemble a prisoner's dilemma game, such preferences can never be taken for granted in nuclear deterrence. The scale of mutual combat is so destructive, that there will always be doubt as to whether one's opponent really prefers that to anything else as an outcome. The question of course will remain, "What purpose could justify the risk of mutual destruction?" Deterrence is always about deterring something. It does not exist independently of a specific action to deter. When coupled with a specific action, such as an invasion of Western Europe, a theory of deterrence can have meaning. In connection with such general threats as Communist domination or a red menace, deterrence becomes so elastic as to be worse than useless. To see the world in such stark false oppositions as "better dead than red" may very well create the conflict one is trying to avoid.

Zagare may be right that deterrence as a theory is successful when it resembles a prisoner's dilemma game. It may be that we have not had a major war between the superpowers since 1945 because the leaders of the Soviet Union and the United States rationally prefer massive death for both of its peoples to some abstract principle or specific encroachment by its antagonist. Alternatively, it may be that deterrence works best when opponents view the other as slightly irrational, that is, as willing to bring ruin on an unprecedented scale for everyone in order to punish a transgression that has already occurred. It is the peculiar dilemma of citizens living in an age of mutually threatened genocide not to know whether to prefer our leaders rational or not.

STEPHEN JOHN STEDMAN

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POLITICAL ECONOMY

The Evolution and Management of State-Owned Enterprises. By Yair Aharoni (Cambridge, MA: Ballinger, 1986. xvi, 411p. $29.25).

Despite the recent episodes of privatization in some countries, state-owned enterprises continue to play important roles in the world's economies. Their performance therefore is of practical concern to the citizens in many countries. Theoretically, state ownership remains a meaningful alternative to private—especially private corporate—ownership. As such it is of central interest to political economists, particularly to students of democratic capitalism. Aharoni seeks to inform citizens and policymakers about the roles state enterprises play and to help them design better institutions through which to manage state enterprises. He also seeks to develop a theory of state enterprise in democracy (p. 39).

The book's value lies in the quantity and range of information it contains. Aharoni catalogs a large amount of material having to do with numerous aspects of state enterprise within and across countries. Some of the information he presents is dated. For instance, his account of the Austrian experience contains little information about the financial difficulties that that country's state steel combine has encountered or about the political reforms that have been adopted recently in an attempt to ameliorate those difficulties (pp. 84–87). Also, most political scientists will want more information about citizens' attitudes toward state enterprises (e.g., survey results) as well as about the views and activities of political parties and interest groups. As a summary of so many aspects of, and national experiences with, state enterprise, however, Aharoni's catalog is relatively current and comprehensive.

Aharoni's discussion rests on some solid analytic footings. For example, his evaluation of state-enterprise performance is rooted in an appreciation of the similarities and differences between public and private firms and also of the character of the industries in which public firms usually operate. As a result, he avoids the mistakes of applying the same performance criteria to both kinds of firms and of assuming that private managers always perform efficiently. In addition, throughout much of his book Aharoni is sensitive to the fact that government is not a monolithic agent with well-formed (social) preferences. He recognizes that government is a hierarchy of competing interest groups and coalitions (p. 376). He therefore avoids the mistake of assuming that efficiency is the sole criterion by which state-enterprise performance should be evaluated.

Aharoni makes some lasting contributions to the study of state enterprise. He dispels many of the myths about, and refutes, some common, monocausal explanations for state enterprise. He identifies gaps in our knowledge about state enterprise. He illuminates logical tensions between such things as public accountability and the pursuit of long-run efficiency. And he produces collections of (cross-national) generalizations about the formulation of state-enterprise objectives and the determinants of state-enterprise performance (pp. 158-60, 215-16).

In some respects, Aharoni's analysis lacks rigor, and the value of some of his contribution is somewhat limited. For example, Aharoni argues that state enterprise is subject to both more and less public scrutiny than private enterprise (cf. pp. 56, 388 with 41, 215, 373). The conditions under which state-enterprise operations are more "visible" than the operations of private enterprise and hence subject to more government intervention need to be more carefully delineated and analyzed. Also, Aharoni's review of existing empirical studies of state enterprise is not synthetic. He makes little effort to determine the extent to which the conflicting results of extant studies of state-enterprise performance are outgrowths of the use of alternative data sets or statistical techniques, let alone to carry out improved empirical analyses or new, rigorous tests of the generalizations he himself proposes.

Most serious for political scientists is the weak analysis of democratic controls over state enterprise. Aharoni emphasizes citizens' rights, willingness, and abilities to hold state enterprises accountable for their performance (e.g., pp. 56, 194, 251). At the same time, he takes as axiomatic politicians' abilities to use state enterprise to serve their selfish, short-run electoral interests, that is, to exploit citizens' ignorance about the workings and welfare consequences of state enterprise (pp. 37, 40, 67, 237, 388, 403). This is contradictory. We need a more rigorous, in-depth analysis of citizens' roles as collective owners of state enterprises and of elections' roles as mechanisms for executing collective ownership rights. In addition, Aharoni assumes that interest-group politics is a constant source of inefficiency (pp. 149, 406). He does not systematically analyze the relative virtues of promoting state-enterprise efficiency through democratic-corporatist, as opposed to pluralist, forms of interest intermediation or, more generally, consider the possibility that interest-group negotiations reveal coalitions' (citizens') tolerance of inefficiency for the sake of some other goal like distributional equity; this in spite of the frequent references he makes to the experiences of countries with democratic-corporatist political institutions and, again, his expressed commitment to public accountability (p. 251). Rather, in the end, Aharoni favors turning over state enterprise to a set of "good" state managers. He advocates insulating state-enterprise management from electoral and parliamentary politics (chap. 11). Aharoni, like Shonfield and others before him, proposes, as a "model" for state enterprise, technocracy, not democracy as he seemed to promise in the first chapter. Of course, this is a contribution. The point is that the theory of state enterprise in democracy needs to be more fully developed and evaluated before we conclude that a technocratic approach to state enterprise is preferable to a democratic approach.

In sum, for those political scientists who are interested in the phenomenon of state enterprise, Aharoni's book is a useful catalog of information. Indeed, because of the solid analytic footings on which much of the discussion rests, most readers will find the book to be one of the best catalogs of information we now have on this subject. Those political scientists who are interested in building theories of state enterprise, especially theories of state enterprise in democracy, will find it to be useful as well but more for the theoretical challenges it poses than for the analytic insights and findings that it presents.

JOHN R. FREEMAN

University of Minnesota

Superfairness. By William Baumol (Cambridge: MIT Press, 1986. xi, 266p. $20.00).

"Probably the most persistent reason for noneconomists' resistance to our most cherished recommendations on micropolicy," observes Baumol, "is our determined disregard of their implications for distributional equity" (p. 1). Concepts and analytical tools have recently been developed, however, that permit a new understanding of distributional questions. Baumol's aim is to present this literature and to show its usefulness in the evaluation of concrete policy issues. Although he partially achieves his first objective, he falls far short of achieving the second.

In 1967, Foley proposed that a distribution of goods among agents with equal claims be declared equitable if no agent prefers someone else's bundle to his own. This is the notion of an *envy-free* (EF) allocation. If there is only one good, then *equal division* (ED) is the only EF allocation. Then ED is also *Pareto efficient* (PE). Otherwise, ED is not in general PE, but allocations that are both EF and PE usually exist. Indeed the Walrasian allocations obtained by taking ED as initial position enjoy both properties.

The introduction of the concept of an EF allocation constituted an important development in welfare economics for the following reasons. First, it is ordinal; economists' reluctance to use the cardinal concept of utility is the principal reason for their extensive reliance on the notion of Pareto efficiency, which essentially places no restriction on the distribution of welfare. Second, it provides a significant reduction of the set of PE allocations; in fact, in many economies with a large number of agents, only the Walrasian allocations from ED, which are generally few, are both EF and PE. Third and more importantly, it is a very natural concept of equity in situations where agents can meaningfully exchange positions, and its recommendations are in accordance with intuition. (To illustrate, an allocation at which one agent receives more of all goods than some other agent is obviously not EF.) Also, it does not rely on ad hoc notions such as, for instance, that of implicit incomes; the suggestion often made by economists to select allocations at which implicit incomes are equal across agents, that is, to allocate resources by operating the Walrasian correspondence from ED, is unnatural since that correspondence has no more normative appeal than a number of other correspondences commonly discussed, when operated from ED.

It was quickly discovered, however, that EF and PE allocations may fail to exist in production economies (because of possible differences in agents' productive abilities). This disappointing result led to the development of an important literature devoted to the formulation and the evaluation of other fairness concepts. Unfortunately, none of them enjoy all of the appealing properties of Foley's original concept.

Baumol does not attempt to review the whole fairness literature, but his exposition of the basic concepts (chaps. 2 and 3) is clear and well illustrated with Edgeworth boxes, a tool ideal for this purpose. He describes the intricate structure of the set of EF allocations in the two-commodity, two-person case, relates the EF concept to the criterion of Pareto domination of ED (an allocation may be EF without Pareto-dominating ED; an allocation in the core from ED may not be EF). He also discusses the problem of evaluating the equity of a trade vector from a given initial position (it is natural to say that a trade vector is envy free if no agent prefers someone else's component to his own), and he notes certain important difficulties that arise when the concepts of EF allocation and EF trade are combined (for example, an EF and Pareto-improving trade from an EF allocation may lead to an allocation with envy). This sort of result has serious implications for the step-by-step implementation of distributional objectives. In this regard, his own attempt at devising a procedure that would yield a single-valued selection from the set of EF allocations via certain sequences of EF trades is instructive, in spite of it being largely unsuccessful. Finally, the author presents, although in much less detail, various generalizations of the EF concept to production economies.

On the other hand, the author's efforts to show the relevance of the fairness concepts to policy issues are unconvincing. His analysis of rationing schemes (chap. 4) yields no useful results, and his treatment of arbitration procedures (chap. 11) is very limited. (The latter would have been considerably helped by a more systematic use of the Edgeworth box representation, a technique also perfectly suited to this topic.)

In spite of these serious flaws, however, the chapters discussed thus far are well unified by a common conceptual apparatus. They could have constituted the core of a worthwhile monograph. Unfortunately, the other chapters (a large portion of which are reprints from the author's previous works) devoted to policy issues such as conservation (chap. 5), industry pricing (chaps. 6 and 7), peak and off-peak pricing (chap. 8), and income equalization (chap. 9), are very disappointing (I will not discuss chap. 10, concerned with certain methodological issues pertaining to "economic illusion": this chapter, also very weak, stands somewhat apart). The analysis is conducted mainly in terms of the standard concepts of Pareto efficiency and Pareto domination. Chapter 7 does contain a fairly comprehensive, although superficial, review of various normative theories of cost allocation schemes, but an application of the concepts and methods of the first chapters is seriously attempted only in a section concerning utility pricing. However, apart from the fact that nothing specific is really said about utilities, the use of the EF concept to analyze a situation in which the two agents are a representative consumer and a representative firm and the two goods are a physical resource and a price is artificial. In fact, it illustrates the basic difficulty (discussed in chap. 11) with the application of the fairness concepts; their usefulness seems to be limited to situations in which all agents play fundamentally symmetric roles.

WILLIAM THOMSON

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The Reason of Rules: Constitutional Political Economy. By Geoffrey Brennan and James Buchanan (Cambridge: Cambridge University Press, 1985. xiv, 153p. $34.50).

Constitutionalism is back. We see it return in a variety of forms. One version is the constitutional political economy developed by the "Virginia school" of public choice. The methodology of such a constitutional political economy is the subject of the present book. But there are many other types of constitutionalism now in the works, including forms of a constitutionalist political economy very different from the Virginia one. These alternatives include applications of interpretive methods to the most fundamental and constitutive political questions as well as the more conventionally scientific study of how impartial standards can restrain collective decisions. There are hermeneutic, neo-Straussian, neo-Parsonsian and causal-behavioral constitutionalisms, as well as the public-choice version under review here.

Why is constitutionalism back? To a degree, no doubt, it is simply a fad, fueled perhaps by the approach of the 200th anniversary of the great constitutions (the U.S., 1787; the Polish, 1791; the French, 1793). But it is surely more than a fad. It is a response to the current political situation, especially the crisis of the "democratic welfare state." This motive is quite clear in the book under review. But it is also a response to the crisis of political, social, and economic theory, an effort to redirect it to more fundamental questions and to questions of greater normative significance.

There is no question that the Virginia school of public choice is now and will continue to be an important player in this revival. The works of Buchanan, Brennan, Vanberg, and others define a distinct constitutionalist alternative. Anyone who takes the new constitutionalist renaissance seriously will have to study them carefully. Brennan and Buchanan's *The Reason of Rules* can best be evaluated in this context. It is important as a report of the current stage of development of the "Virginia" alternative. But it is unlikely, in my judgment, to be a more definitive statement. The book has many marks, in fact, of a transitional work. It is short and sketchy. Most of its arguments are not worked out in great detail, many are short summaries of more elaborate presentations elsewhere, without any attempt to achieve a "grand synthesis." The tone of the book is highly tentative, it is full of *maybes* and *mights*. The overall organization is rather weak; there are at least two dramatic and unexplained shifts in argument. Finally and most importantly, Brennan and Buchanan's book defends a position that, by the end, the authors themselves acknowledge to be self-contradictory.

The Reason of Rules is engaged in two important intellectual battles. It defends a morally skeptical, contractarian position according to which politics is the pursuit of self-interest analogous to exchange in the market. It attacks the view that politics involves the search for right answers, that it is more like the work of scientists or members of a jury. But the book is simultaneously engaged in another kind of dispute. It sets out a case for the constitutionalist position, for the importance of choosing relatively rigid rules that will provide constraints for day-to-day decision making. It bases this case on the empirical hypothesis that people are more "future oriented" in their private decisions than in their public ones. Hence, without constitutional rules collective decisions will be shortsighted relative to individual decisions. Among possible consequences of this pattern Brennan and Buchanan discuss excessive taxes, growing public debt, and inflation. In the end, these two positions, the morally skeptical and the constitutionalist, come into conflict. If politics is only the rational pursuit of self-interest, then the constitutionalist argument cannot succeed politically, because it cannot succeed (as the authors note) by appealing exclusively to self-interest. One of the two positions must give way. Let me offer a suggestion: in order to maintain their "public-choice" position, Brennan and Buchanan need only assume that "politics as exchange" is ordinarily dominant over "politics as science"; they do not have to assume the latter does not exist. If we allow both kinds of politics in our theory, we can be more optimistic about the future of constitutionalism and about the future of democracy as well. But we also have a new task: to understand that aspect of politics—however weak—that is best seen as the search for a right answer. It is an aspect of politics that seems to me far less well understood than "politics as exchange." And it requires something very unlike economic theory to understand it.

KAROL EDWARD SOLTAN

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Electoral Laws and Their Political Consequences. Edited by Bernard Grofman and Arend Lijphart (New York: Agathon, 1986. xiv, 335p. $38.00, cloth; $16.50, paper)

In an era of renewed legal and scholarly interest in the subject of democratic representation, Bernard Grofman and Arend Lijphart have collected 19 articles—18 previously unpublished—in a useful volume on the impact of electoral laws. The volume, which includes a very good bibliography and index, is divided into four parts: the first part (chaps. 1-5) examines the impact of election format on political competition; the second (chaps. 6-10) reviews systems of proportional representation (PR) and semiproportional representation; the third (chaps. 11-16) considers various aspects of plurality systems; and the fourth (chaps. 17-19) looks at redistricting.

The approach of the book is broadly comparative in nature, with 11 of the 19 essays dealing in whole or in part with electoral practices outside the United States. The first three chapters examine the validity of Maurice Duverger's "laws" on the relationship between electoral format and party competition. William H. Riker (chap. 1) and Giovanni Sartori (chap. 2) agree that Duverger's original formulations are "ambiguous" (pp. 20, 44), but Sartori disputes Riker's reformulation of the principal law. Sartori lays out a careful analysis of the conditions that give rise to various party systems and concludes with two "tendency laws" that are, in Sartori's words, "just about all that Duverger said": "Plurality formulas facilitate . . . a two party format and, conversely, obstruct multipartyism" and "PR formulas facilitate multipartyism and are, conversely, hardly conducive to two-partyism" (p. 64). Duverger himself (chap. 3) reviews party competition in Western European countries in the four decades since he first proposed his "sociological laws" (p. 70).

A second group of the "comparative" chapters (4, 6-7, 9-11, and 18-19) deals primarily with various aspects of PR abroad; these chapters cover such matters as national and state representational formulas in Australia; semiproportional representation (limited voting) in Japan and Spain; and special provisions for ethnic representation in Belgium, Cyprus, and several other countries. Lijphart (in chap. 10) cogently examines the different forms of PR, explaining the formulas and ranking the various approaches in terms of the correspondence of party representation to party vote. In a related article, Peter Taylor et al. review the "geography of representation."

For the most part, the "comparative" chapters offer a very readable, and sometimes eye-opening, introduction to foreign election systems. An example is Richard S. Katz's "Intraparty Preference Voting." While pointing to the direct primary in the United States as the best example of voter control over the selection of party candidates, Katz notes that "the majority of democratic electoral systems . . . allow those voting for a party to determine, or . . . to influence, the identity of the particular candidates from among those the party has nominated." In a number of countries—Finland and Italy among them—*intra*partisan defeat created more parliamentary turnover than did *inter*partisan defeat. Peter Mair's article on Ireland, where members of the Dàil are elected from multimember constituencies by the single transferable vote (STV), notes that candidates often seek election through constituent service to "particular bailiwicks" (pp. 293-94). Students of reapportionment in this country will be amused by R. J. Johnston's article on recent redistricting of the British Parliament. British courts in 1982-83 upheld the plan adopted by the Boundary Commission; in the process, the courts refused to establish a precise "definition of excessive discrepancy" (p. 282) in the population of legislative districts and, at the same time, recognized that (under statutory guidelines) "equality of representation for *people* is secondary to that of representation for *places*" (p. 284).

Among the articles focused primarily on the United States are selections on format of partisan ballots and candidate cross filing. Three deal primarily with the consequences of the Progressive movement of the early twentieth century. Leon Weaver traces the history of PR (usually STV) in the United States. Twenty-two city councils experimented with PR, mostly in the period between the two world wars, but by the late 1950s PR had nearly disappeared. In recent years PR has attracted renewed interest. Richard L. Engstrom and Michael D. MacDonald address the impact of at-large (as opposed to ward or mixed) elections on minority representation on city councils. The thesis that blacks are less likely to achieve "proportionate" success under at-large than under ward elections is, they conclude, "impressively documented" (p. 224). However, they find the evidence of the linkage between black representation and governmental responsiveness to minority interests to be ambiguous. Interestingly, in her review of the effects of nonpartisan elections (as compared to partisan elections), Carol A. Cassel suggests that a nonpartisan format "inhibits the election of black council members but promotes the election of black mayors" (p. 237).

Among the other articles on the United States, Gordon E. Baker evaluates the impact of the reapportionment and calls for "a long-overdue 'dialogue' about the fundamentals of representation" (p. 274). Arguing that two decades of court-supervised redistricting activity has brought "only a partial victory" (p. 266) in the battle for "fair representation," Baker criticizes the Supreme Court's preoccupation with strict mathematical equality in legislative districts to the neglect of broader questions of representation: "Finding ways to check partisan gerrymandering that has proliferated in the wake of one-person, one-vote rulings is probably the most pressing challenge" (p. 274).

Baker's essay raises questions about representational values that merit more attention than they receive in this volume. Litigation against at-large elections and partisan gerrymandering, for instance, has focused attention on political scientists' conflicting interpretations of the consequences of particular election rules. Yet taken as a whole, this collection gives too little attention to the substance—and drama—of the controversy surrounding "fair representation." In this respect, one wishes that the spirit of debate that marks the opening chapters might have been extended to other parts of the book. Then too, the editors' introductory essay might have more clearly identified the underlying issues that connect the wide-ranging subject matter of the various articles. Two or three of the articles are too brief, and thus sketchy; conversely, too much space (about one-fifth of the text) is devoted to the three chapters on Duverger.

Yet, on balance, this is a worthwhile collection. In the editors' words, this volume is supposed to serve as "an overview of recent research on electoral laws . . . by scholars who have helped shape the field" (p. 1). The volume surely achieves this modest objective. In doing this, the book establishes a broader international and interdisciplinary perspective on the methods of representation.

TIMOTHY G. O'ROURKE

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Who Profits: Winners, Losers, and Government Regulation. By Robert A. Leone (New York: Basic Books, 1986. xiii, 248p. $17.95).

The last decade has witnessed a revolution in the regulatory process that rivals the New Deal in scope and impact. For the first time in history, the executive branch has achieved an unprecedented degree of control over the vast body of regulatory activity in both the independent and executive agencies. Initiated by the Carter administration, and accelerated by President Ronald Reagan, the new regulatory process is as different in its treatment of administrative power as were President Roosevelt's initiatives in the 1930s. Now, however, the role of government has changed. In the New Deal period, the president viewed government as a growth industry, to be used as a vehicle for problem solving; while today, White House efforts focus on minimizing government, in order to allow market forces to work more effectively.

Who Profits is an example of the kind of thinking, much in vogue in the late 1970s, that led to this revolution in executive power. It is primarily a study of the negative impacts of regulation on the manufacturing process, using examples that draw heavily on case materials from the Harvard Business School. The author is an economist who understands the problems facing manufacturers suddenly confronted with regulatory decisions that can drive them out of business or cut substantially into their profit margins. He analyzes the impact of regulatory decisions on such factors as differential costs, tracing what happens to affect the final drafting of the bottom line. Especially interesting is his discussion of regulation as a disincentive to new investment; how the existence of a cumbersome regulatory process leads to a bias against building new manufacturing facilities, and affects U.S. competitiveness. Leone's extensive use of the vast literature on the costs of regulation brings this body of work up to date and casts a critical eye on some of its problems.

Actually, *Who Profits* could serve as a road map for what has already transpired in the federal government, although the author does not seem aware of these developments. In that sense, the book seems out of date. The author argues eloquently for the use of economic analysis in regulatory decision making on the grounds that it does not exist in the federal arena. In fact, economic analysis has been mandated in all major regulatory decisions since 1981 and the promulgation of Executive Order 12291. The order is rigorously enforced by the Office of Management and Budget, and the agencies have heard the message loud and clear. Benefit-cost analysis now supersedes technical and scientific analysis, as well as health, safety, and environmental considerations.

The book's theoretical approach posits a view that looks very much like public-choice theory. Briefly, Leone argues that public-policy decisions have an enormous impact on industry, that there are heavy winners and losers, and that policymakers need to be made more aware of the economic consequences of their decisions. A strong faith in the value of marketplace competition is repeatedly expressed throughout the book. The idea is that the forces of the free market almost always lead to outcomes preferable to government intervention. There is virtually no recognition of the unanticipated negative consequences of untrammeled market forces or of what happens when an unregulated marketplace is overtaken by predators out to destroy competition. Scholars needn't go far to find examples of this outcome: ample evidence exists in the fields of antitrust, airline, trucking, and railroad deregulation.

Another important subtheme emphasizes that regulatory decision makers are unaware of the unanticipated consequences of their decisions. To buttress this argument, the book offers many examples, most of which fall into one category: the impact on a firm or an industry's profit margins. Some of these examples are rich in detail and offer dramatic proof of the costs of regulation. Other examples show inadequate documentation or inaccuracies. The author, in attacking liberals for ignoring the long-term effects of government intervention, writes, "When policy makers for the state of California look at the prospect for a unitary tax. . . ." California has not looked at the "prospect" for a unitary tax for a long time; it has had a unitary tax since 1929, repealing it in 1986. When dealing with auto-safety regulation, the book concludes that there are more "cost effective ways of achieving levels of safety benefits than passive restraints." The documentation for this statement is a General Motors report.

The book's strength is its appreciation for some of the economic problems incurred by industry as a result of its relationship to government. Its weakness lies in its treatment of the political and regulatory process, and its lack of understanding of those processes. The argument that public managers "tend to be held accountable almost exclusively to process and rarely to outcome" misses the importance of integrity to the regulatory process: if the process is fair, there is a greater chance that the outcomes will favor the public's interests. This is the basis for the Administrative Procedure Act and for appeals by citizens and corporations who feel that they have been disadvantaged by regulatory decisions.

The author's major concerns are competitive realities, and he deals with these very well. He skillfully disentangles the roles of equity and efficiency, emphasizes the importance of business managers' understanding the regulatory process, and gives solid advice to industry on how to manage the regulatory process in a more sophisticated way. "Take preemptive action," "Take individual action," and "Use information as a weapon," are several suggestions that are offered corporate public-affairs managers. Public managers are advised to manage their data better and to intrude on industry as little as possible.

But regulatory problems have a broader dimension, as practitioners and scholars are beginning to recognize. What happens in the absence of regulation, and what are the long-term costs on this "bottom line" approach? Who suffers? and Who pays in the long run? are questions notably missing from the Who-profits? approach. In fact, new technologies need more government regulation, not less, as their leaders are beginning to recognize: recent efforts by the biotechnology industry to seek additional government regulation bears witness to this change in thinking. That process needs to be fair, cost conscious, and adequate to the task, in order to help industry fulfill its potential and to protect the consumer. And, as government and enlightened business leaders agree, regulators need to be at least equal in strength to the industry they are regulating, not subordinate to the current vogue of market forces.

SUSAN J. TOLCHIN

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Game Theory and Political Theory. By Peter C. Ordeshook (Cambridge: Cambridge University Press, 1986. xv, 511p. $49.50, cloth; $17.95, paper).

Many of us have been awaiting publication of Peter Ordeshook's *Game Theory and Political Theory* for quite a few years. Now it has arrived, and the wait has been worthwhile. As the subdiscipline generally known as positive political theory enters its third decade, Ordeshook has crafted what for the time being must be regarded as the definitive survey of the field. The treatment is comprehensive (within the generous limits of five hundred pages) with respect to the theoretical substructure of the field and with respect to applications of that theory to electoral and committee voting processes (though bureaucratic and nondemocratic processes are not covered); up-to-date; and—given the author's many contributions to the literature, both theoretical and experimental, that he is reviewing—distinctly authoritative.

Ordeshook's premise—and what his book demonstrates—is that "formal models of politics" now form an integrated whole, much like "formal models of markets." It is game theory that unifies the heretofore disparate models into what Ordeshook boldly calls "political theory" (dropping the modifier *positive*). From this premise come both the title and the organization of Ordeshook's book.

Ordeshook's first chapter presents an essentially standard, but exceptionally clear and comprehensive, discussion of individual preference and choice. In the first part of chapter 2, he presents Arrow's impossibility theorem and a variety of other voting paradoxes, but to this point he assumes that electoral institutions mechanically aggregate individual preferences into social choices. In the second part of the chapter, Ordeshook establishes the possibility of strategic manipulation of institutions, first by considering the power of agendas in influencing voting outcomes and second by considering incentives for misrepresentation of preferences under voting procedures and social-choice functions of all types. This sets the stage for the systematic analysis—provided by game theory—of strategic interaction of agents within institutions. The remaining eight chapters then survey standard topics in game theory, but with interesting differences in organization and emphasis compared with the classic texts in the field.

Indeed, Ordeshook's book invites comparison with R. Duncan Luce and Howard Raiffa's *Games and Decisions* (New York: Wiley, 1957), which for years has been considered the standard treatment of game theory for social scientists. It would be fair to say the two books are pitched at about the same level of abstraction and difficulty. And Ordeshook's writing approaches the standard of clarity set by Luce and Raiffa (quite a compliment). To some degree, Ordeshook shares the critical posture of Luce and Raiffa, at least to the extent of regularly pointing out that much conceptual development and research remains to be done in the construction of comprehensive political theory. At the same time, there are major differences between the two books, which derive from two obvious and related facts. The first is the Luce and Raiffa book is now 30 years old; while the main lines of game theory were clearly established when they wrote, there have been many very significant developments in game theory itself (especially new solution concepts) and in its application over the past decades. Second, Luce and Raiffa were not political scientists, and their survey did not focus particularly on applications of game theory to politics. Indeed, apart from a discussion of the Shapley-Shubik power index to weighted voting systems and a brief and unsystematic discussion of strategic aspects of majority rule, their survey includes no distinctively political topics. Of course, at the time they wrote there was little to survey in this respect, while many of the more recent applications of game theory are to political processes.

Ordeshook's focus on political applications of game theory leads him to depart from the standard mode of presentation established by Luce and Raiffa. Conventionally, two-person games are considered before *n*-person games, and zero-sum games before variable-sum games, but Ordeshook moves directly (in chap. 3) to consider *n*-person variable-sum noncooperative games (focusing on voting games in particular). And conventionally, cooperative games are considered under the assumption of transferable utility (generally relevant to economics but not politics), while Ordeshook treats transferable utility only as a very special case. Perhaps most important, Ordeshook devotes a great deal of attention (beginning in chap. 1) to spatially defined preferences, on which most models of electoral competition and committee voting are now based. This orientation is completely missing from Luce and Raiffa and most other game-theory texts.

In addition, Ordeshook's new book invites comparison with his previous text, coauthored with William Riker, *An Introduction to Positive Political Theory* (Englewood Cliffs, NJ: Prentice-Hall, 1973, now out of print). The new book is no mere updating, as it presents a far more coherent synthesis of the seemingly distinct topics that defined the chapters of the earlier book. And of course the new book reflects the progress of research over the past decade (most notably, perhaps, with respect to the theory of spatial voting games and agenda processes and with respect to experimental testing of solution concepts).

Game Theory and Political Theory will serve two audiences especially well. First, specialists in the area of positive political theory will surely want to read it to see how a leading scholar in the field organizes and presents familiar but difficult material; they will also find it handy reference work with respect to recent and perhaps unfamiliar developments. Second, present nonspecialists who are seriously interested in gaining an understanding of positive political theory will find this an excellent place to start. In particular, the book will serve as a fine text for graduate-level courses in the field. (I doubt it can be successfully used at the undergraduate level). The fact that a literature guide is attached to each chapter together with the fact that Cambridge is publishing the book in a reasonably priced paperback edition, will enhance the book's utility as a course text.

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Coalitional Behaviour in Theory and Practice: An Inductive Model for Western Europe. Edited by Geoffrey Pridham (Cambridge: Cambridge University Press, 1986. xix, 308p. $44.50).

The recent revival of interest in government coalitions in parliamentary democracies is indisputable. Geoffrey Pridham's recent book underscores the rediscovery of this traditional topic among Western Europeanists. However, this is not another collection of theoretically unconnected, country-specific articles. Nor is it a volume of abstract game-theoretic exercises. On the contrary, the purpose of Pridham's book is to present a sustained critique of, and alternative to, the game-theoretic approach to parliamentary government. Pridham refers to this enterprise as a fresh look at coalition theory in the light of coalition experience (p. xvii) and calls his alternative an inductive theoretical framework within a multidimensional perspective.

This edited volume is the fruit of a workshop held during the sessions of the European Consortium for Political Research at Salzburg in April 1984. The volume is implicitly divided into three parts. The first two chapters (by Pridham and Michael Laver) outline the analytical framework and critique the existing literature. The bulk of the volume (the next nine chapters) consists of country studies more or less committed to the analytical tools Pridham provides in the first chapter. Finally, chapters 12 and 13 explore the applicability of the same tools to coalitions at subnational levels and introduce the reader to a couple of important ongoing research projects. Neither of the latter, however, bears much theoretical resemblance to Pridham's approach.

In the Preface, the editor informs us that the idea for the volume originated in his own work on Italian coalition politics. In fact, Pridham's chapter on Italian coalitions is a good place to begin familiarizing oneself with his approach to coalition formation and maintenance. Although the chapter is neither parsimonious nor particularly elegant, it does demonstrate the relevance of his seven dimensions to the study of coalition politics. More specifically, Pridham shows how the governmental game in Italy is constrained by historical events (such as established coalition formulae), institutional characteristics (e.g., the role of the president), differences in party motivation (patronage vs. policy), local and regional coalition patterns (often precursors of national coalitions), internal party factionalization (most notably among the Christian Democrats), sociopolitical factors (e.g., declining mass deference and party identification), domestic events (stagflation), and international commitments (e.g., to NATO and the United States).

Pridham's attempt in the introductory chapter to deal with these constraints analytically is much less persuasive. The discussion is frequently rambling and repetitive. Pridham presents no sustained critique of the deductive theories he opposes, and his own model is not presented with great clarity. His invocation of the concept of dimensionality is confusing. Deductive coalition theories are labelled "one-dimensional," but this somewhat mystifying characterization is never explained to the reader. Nor is it clear what is dimensional about Pridham's own dimensions, which seem more aptly described as constraint categories. Moreover, one must question the editor's contention that his inductive approach "is imperative for handling the complexity of this subject in a viable and comprehensive way" (p. 14). Complex subjects may require complex models, but not necessarily inductive ones. These flaws notwithstanding, Pridham deserves credit for his ambitious attempt to bring a broad range of literature to bear on the subject of coalition formation and maintenance.

The reader is treated to another aperitif in the form of Michael Laver's analytical chapter on deductive models and cabinet coalitions in Europe. This concise, witty, and admirably nontechnical chapter is clearly the best in the book. It should be especially instructive for the intelligent novice to this field of study. Like Pridham, Laver criticizes formal coalition theories for a number of unrealistic assumptions. Foremost among these problems is the static nature of most theories. However, Laver's conclusions are more sanguine, in the sense that he sees more potential for progressive modifications of deductive coalition theory.

Laver is also partly responsible (with Michael D. Higgins) for a very readable and informative chapter on government coalitions in Ireland. The country chapters are on the whole surprisingly even in quality and structure. Certain heterogeneities and imbalances do, however, exist. Two chapters focus on the coalitional behavior of particular parties rather than national party systems. The chapter on France (by Eric C. Browne and Dennis W. Gleiber), on the other hand, scrutinizes a single government (Ramadier) and departs radically from Pridham's multidimensional approach.

Geographical coverage is, as usual, not strictly determined by theoretical interest. Thus it is difficult to see why Germany, with its paucity of parties and coalitions, deserves two chapters, whereas the Scandinavian countries collectively get the short shrift of one. The latter chapter (by John Fitzmaurice) is in turn internally uneven. Denmark is covered in a much more satisfactory manner than Sweden or (particularly) Norway. On the latter country, Fitzmaurice commits a multitude of embarrassing factual errors, such as misspelling the names of all but 2 parties (out of 11) in the Appendix (pp. 276–77). A positive feature of the country coverage is the inclusion of Spain, despite the limited history of party coalitions in that new democracy.

In sum, Geoffrey Pridham and his collaborators have made a useful, but hardly indispensable, contribution to our understanding of coalition governments. The book may be read for its juxtaposition of Pridham's and Laver's critiques of deductive coalition theory. Some readers may also value the insight they gain concerning ongoing research projects. Yet it is fair to assume that most will find its main contribution in the rich and relatively systematic country-specific treatment of historical, cultural, and institutional constraints on coalition politics.

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Erratum

Francis A. Beer, Alice F. Healy, Grant P. Sinclair, & Lyle Bourne, Jr. "War Cues and Foreign Policy Acts" (September 1987, 701–55). On page 706, the figures should be titled as follows:

Figure 1. Mean Cooperation–Conflict Scale Score ("Conflict Score") as a Function of Dominance Group and Priming Condition

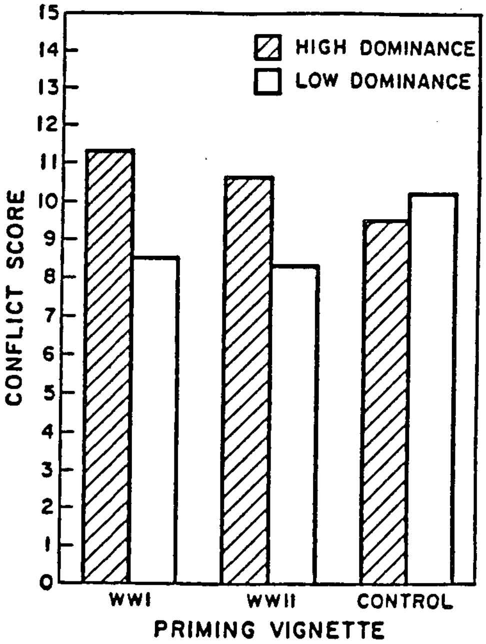
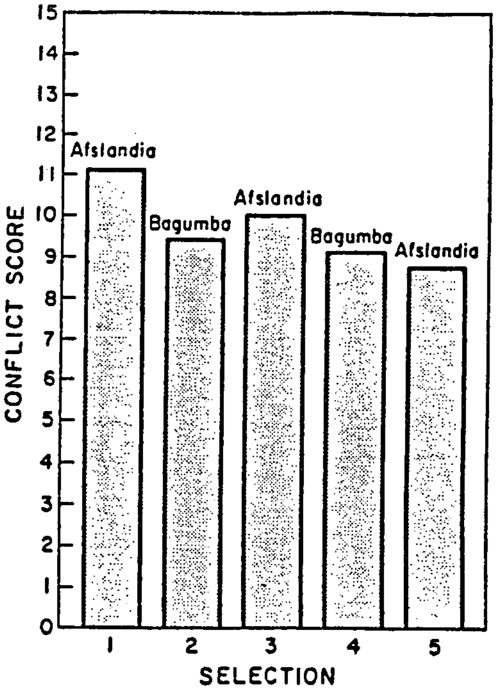


Figure 2. Mean Cooperation–Conflict Scale Score ("Conflict Score") as a Function of Successive Selection



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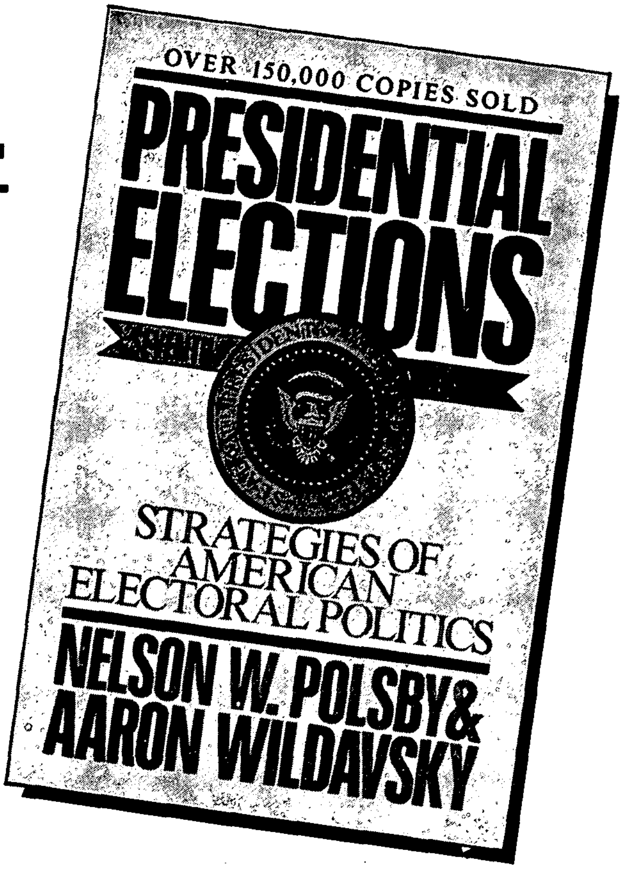
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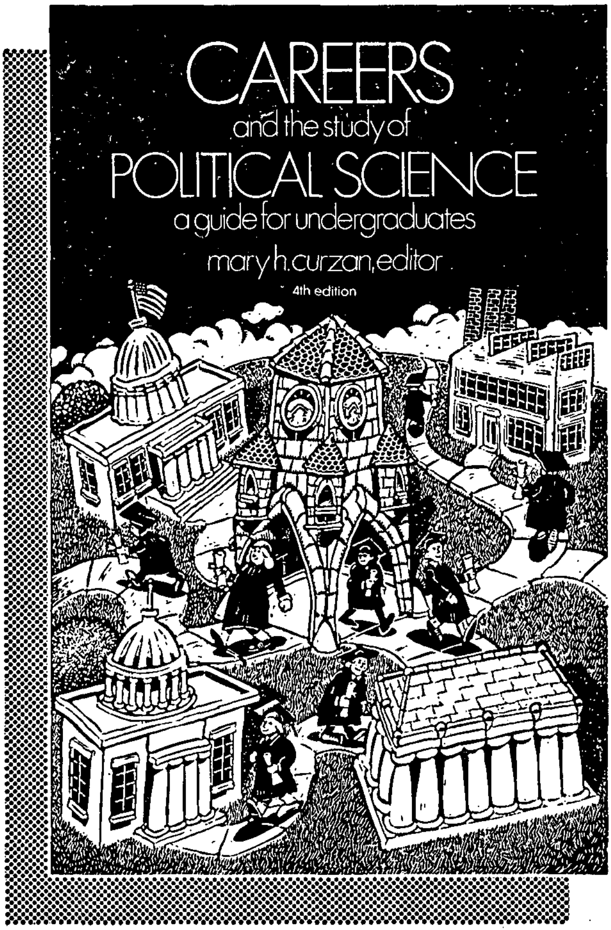
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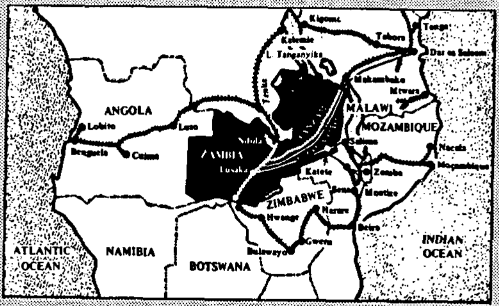
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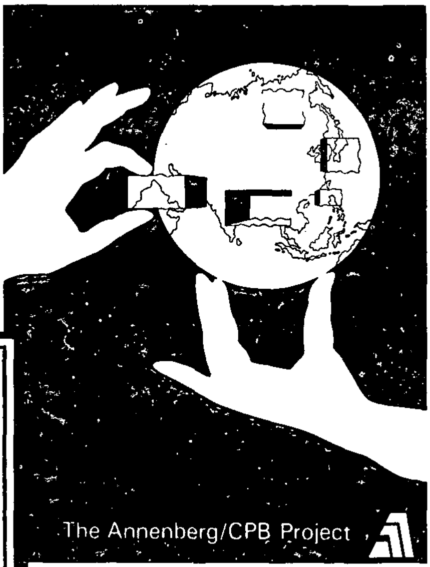

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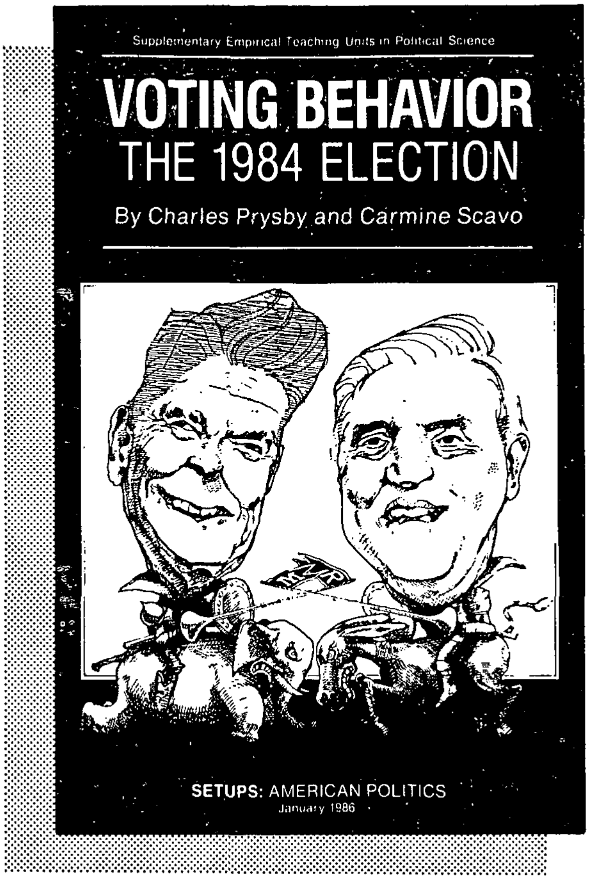

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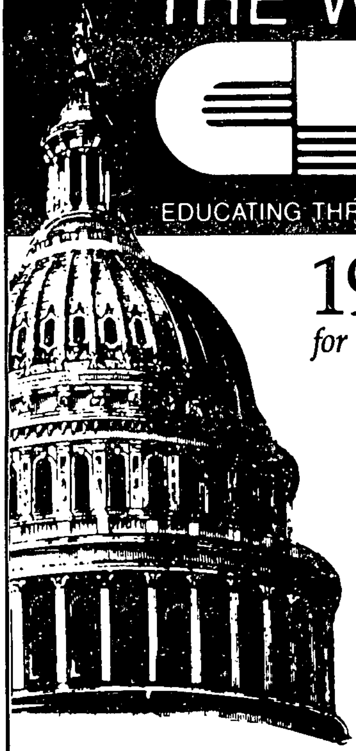

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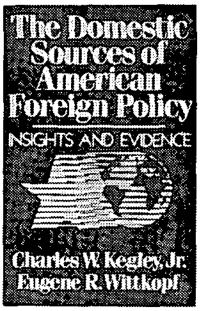
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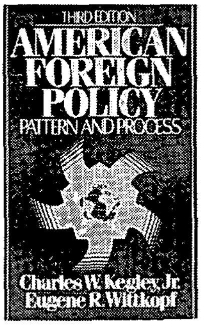
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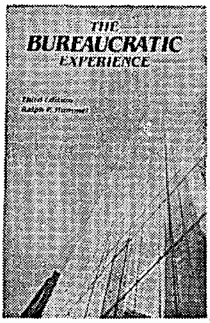

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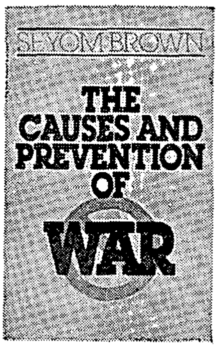

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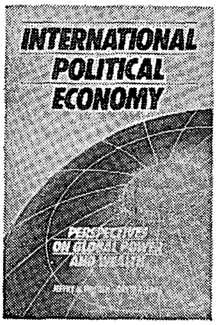
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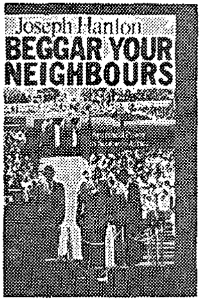

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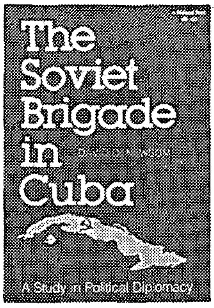

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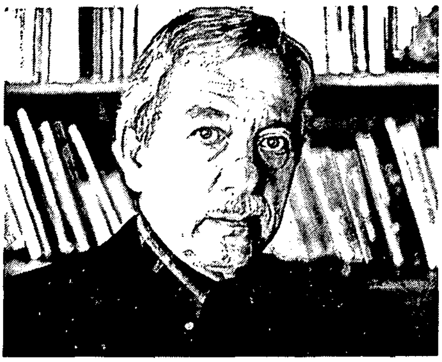

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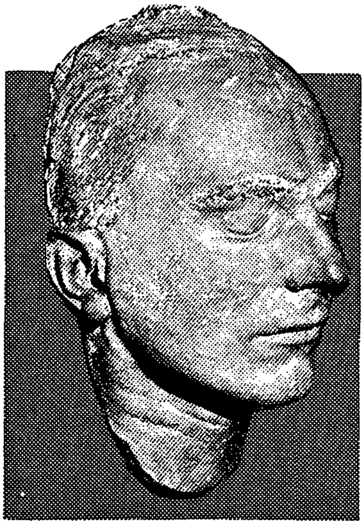

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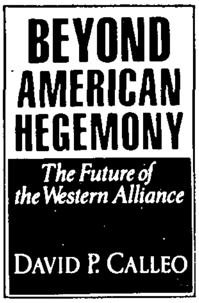
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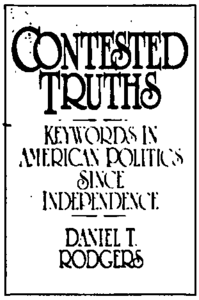
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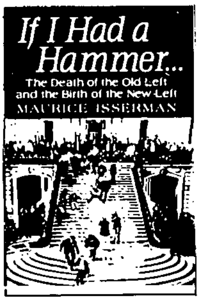
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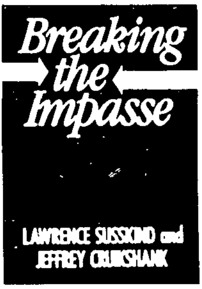
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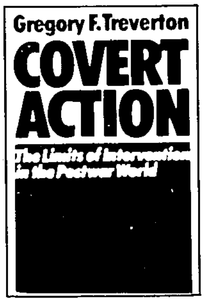
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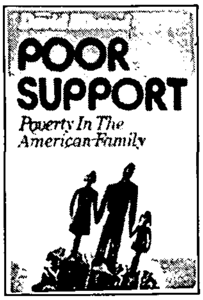
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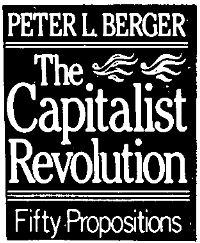
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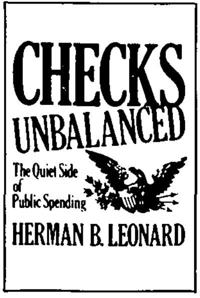
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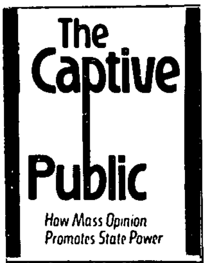
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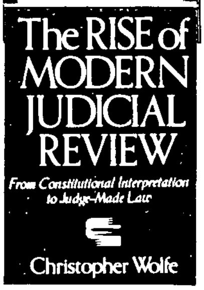
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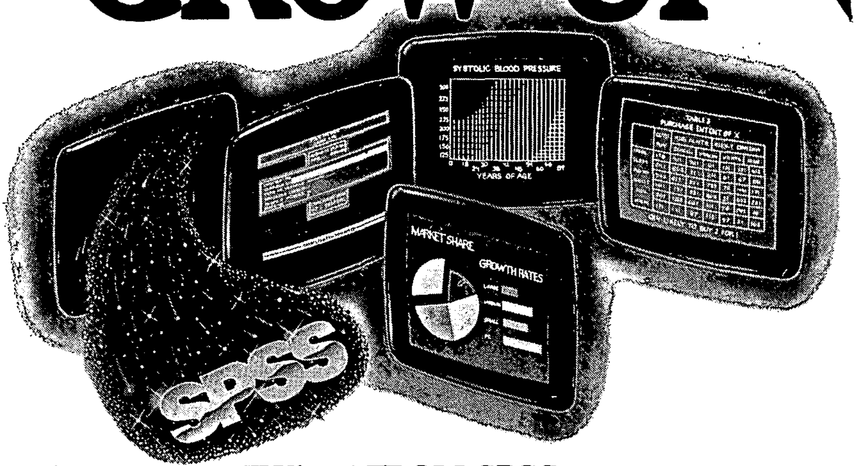

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